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ARTICLES

THE ASSESSMENT OF EXPERTISE: TRANSCENDING CONSTRUCTION

Randolph N. Jonakait*

INTRODUCTION

Daubert v. Merrell Dow Pharmaceuticals, Inc. held that scientific evidence must be based on “good grounds” in order for it to be admitted.

The subject of an expert’s testimony must be “scientific . . . knowledge.” The adjective “scientific” implies a grounding in the methods and procedures of science. Similarly, the word “knowledge” connotes more than subjective belief or unsupported speculation [I]n order to qualify as “scientific knowledge,” an inference or assertion must be derived by the scientific method. Proposed testimony must be supported by appropriate validation—*i.e.*, “good grounds,” based on what is known. In short, the requirement that an expert’s testimony pertain to “scientific knowledge” establishes a standard of evidentiary reliability.¹

To make this determination, a trial court must undertake a “flexible” inquiry into whether the proffer is of truly “scientific knowledge.” “Many factors will bear on the inquiry,” but the trial court should consider whether the theory or technique “can be (and had been) tested [and] whether [it] has been subjected to peer review and publication.”² The trial court should also consider “the known or potential rate of error . . . and the existence and maintenance of standards

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1. *Daubert v. Merrell Dow Pharmaceuticals, Inc.*, 509 U.S. 579, 589-90 (1993).

2. *Id.* at 593.

controlling the technique's operation" as well as the acceptance of the theory or technique in the scientific community.³

In adopting this flexible framework, the Court explicitly stated that it was not deciding how the admissibility of other expert evidence ought to be assessed.⁴ Courts, however, have traditionally employed a dual-level approach to the admissibility of expert testimony. Novel scientific evidence has been subjected to a special scrutiny,⁵ while other expertise has been admitted with little examination. Courts have seldom analyzed whether the expert opinion is based on "good grounds," as *Daubert* requires for scientific expertise. Instead, judges have generally only demanded that a proffered nonscientific expert have appropriate qualifications; if so, the testimony has been allowed as long as it is relevant. In practice, since courts have usually determined that the witness is qualified as an expert,⁶ courts have undertaken little screening of nonscientific expertise.⁷

3. *Id.* at 594.

4. *Id.* at 594-95. "Rule 702 also applies to 'technical, or other specialized knowledge.' Our discussion is limited to the scientific context because that is the nature of the expertise offered here." *Id.* at 590 n.8.

5. *Id.* at 592-93. *Daubert*, however, has ended the traditional distinction between novel and non-novel science. "Although the *Frye* decision itself focused exclusively on 'novel' scientific techniques, we do not read the requirements of Rule 702 to apply specially or exclusively to unconventional evidence." *Id.* at 592 n.11.

6. See Samuel R. Gross, *Expert Evidence*, 1991 WIS. L. REV. 1113, 1158 (1991) [hereinafter Gross] ("[I]f it appears that the witness has at least the minimal qualifications for an expert in the field in which she is offered, she will usually be permitted to [testify]."); Anthony Champagne et al., *An Empirical Examination of the Use of Expert Witnesses in American Courts*, 31 JURIMETRICS J. 375, 390 (1991) [hereinafter Champagne et al.] ("In jury trials, if it is a close question whether an expert is qualified to testify, 80% of the judges were inclined to let the jury hear the testimony."). See also Christopher P. Murphy, *Experts, Liars, and Guns for Hire: A Different Perspective on the Qualification of Technical Expert Witnesses*, 69 IND. L.J. 637, 654 (1994) [hereinafter Murphy] ("[Q]ualification of an expert is within the broad discretion of the trial judge; appellate courts will not disturb such discretion unless the ruling is manifestly erroneous.").

7. See Paul C. Giannelli, *Daubert: Interpreting the Federal Rules of Evidence*, 15 CARDOZO L. REV. 1999, 2010-11 (1994) ("[T]he qualification of the expert presumptively qualifies the technique. This formulation of the relevancy approach makes the trial court too dependent on the testifying expert. Unfortunately, experience shows that qualified witnesses do testify to questionable conclusions.").

Cf. David L. Faigman, *To Have and Have Not: Assessing the Value of Social Science to the Law as Science and Policy*, 38 EMORY L.J. 1005, 1012 (1989) ("The usual response, and in fact the law's apparent course, has been to trust

This article will discuss why this dual-level approach should end and how a more unitary approach to expert evidence might be achieved. Part I discusses how the basic premises and logic of *Daubert*, the text of Federal Rule of Evidence 702, the impracticalities of finding a useful boundary between scientific and other expert testimony, the information available to the jurors for evaluating expert testimony, and conclusions from the sociology of science all indicate that the dual-level approach should cease. Part II discusses how *Daubert* implicitly accepts that science is a path to good knowledge about the empirical world and rejects the view that science is merely a construct on the same footing as other kinds of knowledge. The principles that allow confidence that science is advancing knowledge provide a general framework, discussed in Part III, for assessing all expertise.

PART I

A. *The Text of Rule 702*

Daubert's most fundamental conclusion is that scientific expertise must be more than just relevant and not unfairly prejudicial to be admissible. Instead, the trial judge must specially filter scientific evidence. This is so, *Daubert* concluded, because Federal Rule of Evidence 702 authorizes the admission of "scientific . . . knowledge," and that term implies something beyond relevance. "[T]he word 'knowledge' connotes more than subjective belief or unsupported speculation. The term 'applies to any body of known facts or to any body of ideas inferred from such facts or accepted as truths on good grounds.'"⁸

The one sentence of Rule 702, however, does more than specify when scientific evidence is admissible. It defines the basic conditions necessary for the admission of any expertise: "If scientific, technical, or other specialized knowledge will assist the trier of fact to understand the evidence or to determine a fact in issue, a witness qualified as an expert by knowledge, skill, experience, training or education, may tes-

certain professional fields to decide which findings can be of assistance. This practice results in the presumptive admissibility of the testimony of any accredited expert whose testimony is otherwise relevant.").

8. *Daubert v. Merrell Dow Pharmaceuticals, Inc.*, 509 U.S. 579, 590 (1993) (quoting WEBSTER'S THIRD NEW INTERNATIONAL DICTIONARY 1252 (1986)).

tify thereto in the form of an opinion or otherwise."⁹ A witness may, thus, testify as an expert when three conditions are met: (1) there is distinctive knowledge, (2) which will aid the jury, and (3) the witness is a qualified expert. The text does not vary these prerequisites depending on the type of expertise, but demands them equally of all.

The term "knowledge" appears only once. The most straightforward textual reading means that if the definition of "knowledge" requires a trial court to screen scientific evidence to make sure that it is based on "good grounds," as *Daubert* commands, then a court should have to sift other purported expert knowledge to make sure that it too is not merely a subjective belief, but instead is based on good grounds.¹⁰ If the opinion does not rest on good grounds, then it is simply not "knowledge" within the meaning of Rule 702.¹¹

While what constitutes "good grounds" may vary depending on whether the proffered expertise is scientific or not, the Rule does not authorize nonscientific expertise merely because the testimony will aid the jury and the witness has the

9. FED. R. EVID. 702.

10. See David L. Faigman, *Mapping the Labyrinth of Scientific Evidence*, 46 HASTINGS L.J. 555, 559 (1995) ("*Daubert* could be read to apply only to 'scientific knowledge,' given the Court's heavy reliance on the definition of the word science in both its dictionary sense and its more philosophical sense. Such a reading displays a crabbed interpretation of the Court's opinion as well as a misconstruction of the principles underlying Rule 702."). Compare Thomas M. Horner & Melvin J. Guyer, *Prediction, Prevention, and Clinical Expertise in Child Custody Cases in Which Allegations of Child Sexual Abuse Have Been Made: I. Predictable Rates of Diagnostic Error in Relation to Various Clinical Decision-making Strategies*, 25 FAM. L.Q. 217, 246 (1991) ("Few courts or lawyers probe the basis for experts' claimed expertise beyond examining their superficial and often-inflated resumes and curricula vitae, . . . but intense scrutiny of the foundation of claimed expertise ought to be standard.") with Michael H. Gottesman, *Admissibility of Expert Testimony After Daubert: The "Prestige" Factor*, 43 EMORY L.J. 867, 878 (1994) ("When a highly qualified scientist is testifying within the specialized field to which she devotes her out-of-court career, her opinions should be admitted without further inquiry.").

11. Cf. Stephen J. Morse, *Failed Explanations and Criminal Responsibility: Experts and the Unconscious*, 68 VA. L. REV. 971, 979 (1982) ("For a science to be the basis of expert testimony, it does not have to be as precise or validated as the laws of motion, but specialized knowledge can assist the trier of fact only if it exists."); Susan R. Poulter, *Daubert and Scientific Evidence: Assessing Evidentiary Reliability in Toxic Tort Litigation*, 1993 UTAH L. REV. 1307, 1321 (1993) ("Invalid reasoning and methods cannot result in probative conclusions; thus, evidence that is derived through invalid methods or reasoning is not relevant.").

qualifications of an expert. The third qualification must be met. If the text and *Daubert* indicate that the trial judge must filter scientific evidence, then they also indicate that the trial judge must screen nonscientific expertise for distinctive "knowledge," that is, facts or ideas based on good grounds, before it can be admitted.

B. *The Difficult Demarcation Line*

If the dual-level approach to Rule 702 with sharply different treatment of scientific and nonscientific expertise were to continue, the line demarcating "scientific evidence" becomes crucial.¹² That boundary, however, is so difficult to draw and can lead to such strange results that it should not be constructed unless truly necessary.

Perhaps the most reasonable definition of "scientific evidence" is that it is information generated by a scientific method (or a scientific method itself). This demarcation, however, reduces *Daubert* to a nullity, for according to that decision, evidence derived from the scientific method is admissible.

The adjective "scientific" implies a grounding in the methods and procedures of science [I]n order to qualify as "scientific knowledge," an inference or assertion must be derived by the scientific method.¹³

Consequently, if *Daubert* were confined to evidence produced by the scientific method, then *Daubert* would only apply to evidence that would be admissible. Evidence not admissible under *Daubert* because, by definition, it is not scientific evidence, would be gauged by some other, presumably less strict, standard and still could be admitted.¹⁴

12. Cf. Richard D. Friedman, *The Death and Transfiguration of Frye*, 34 JURIMETRICS 133, 139 (1994) ("A great deal of the evidence to which Rule 702 applies either is clearly not considered scientific evidence or is close to the line between scientific and other forms of specialized knowledge.").

13. *Daubert v. Merrell Dow Pharmaceuticals, Inc.*, 509 U.S. 579, 590 (1993).

14. See, e.g., *United States v. Starzecpyzel*, 880 F. Supp. 1027 (S.D.N.Y. 1995) (admitting the testimony as "skilled" expertise after concluding that forensic document examination expertise could be scientific, but did not meet *Daubert's* standards). See also *Thomas v. Newton Int'l Enters.*, 42 F.3d 1266 (9th Cir. 1994) ("While a scientific conclusion must be linked in some fashion to the scientific method, . . . non-scientific testimony need only be linked to some body of specialized knowledge or skills."). Cf. Lisa M. Agrimonti, Note, *The Limitations of Daubert and Its Misapplication to Quasi-Scientific Experts*, a

This circularity might be avoided if *Daubert* were limited to proof that a proponent claims is scientific.¹⁵ *Daubert* could then have some meaning, but presumably that meaning would diminish as lawyers learned to label their *Daubert*-questionable proffers as something other than scientific.

If, however, the justification for a dual-level approach to expertise is that jurors have difficulty evaluating science and give it special deference, the *Daubert* strictures could perhaps apply to any evidence that a juror could reasonably understand to be scientific, no matter how the adversaries characterized it.¹⁶ While the next section will contend that the argument over jurors' perception of science cannot justify the dual-level approach, even if it might, a definitional dilemma remains: When is it that jurors reasonably understand evidence to be scientific? That answer is not apparent. Neither *Daubert* nor Rule 702's text gives guidance. As long as *Daubert* is seen as affecting only part of expert evidence, a line-drawing problem exists.

Even if a workable boundary could be drawn based upon juror's perceptions of what is science or what the proponent claims is scientific, that does not mean that it should be. Under such standards, the proffered proof to which *Daubert* will be applied will have some of the trappings of science, for it is such trappings which will lead to the claim or perception of science. If, however, science is the best, or at least a good, method for producing knowledge about the empirical world,¹⁷ evidence which is not good enough science to pass *Daubert*

Two-Year Case Review of Daubert v. Merrell Dow Pharmaceuticals, Inc., 113 S. Ct. 2786 (1993), 35 WASHBURN L.J. 134, 143, 147 (1995):

The testimony is not scientific testimony unless it is based on the scientific method, which means the theory must be tested. Accordingly, if testing is not performed, there is no scientific knowledge. If there is no purportedly scientific knowledge, *Daubert* is inapplicable Issues of reliability in these cases ought to be left to the jury, not to the judge

....

Id.

15. See Lee Loevinger, *Science as Evidence*, 35 JURIMETRICS J. 153, 179 (1995) ("Probably the best rule, and the one that appears to be implied by *Daubert*, is that a witness who purports to base testimony upon scientific knowledge must be tested under the standards stated in *Daubert*.").

16. *United States v. Starzecpyzel*, 880 F. Supp. 1027, 1049 (S.D.N.Y. 1995) (possible prejudice from nonscientific forensic document examiner testimony to be alleviated by instruction to jury that the witnesses offer practical, not scientific, expertise).

17. See discussion *infra* Part II.A.

may still be better information than evidence which has no pretensions to science. The athlete who tries out for the Olympics and fails is probably a better athlete than the one who never seeks to qualify. Data which is close to being acceptably scientific, and might be regarded as scientific by the jury, but fails to satisfy *Daubert*, may be better proof than that which has no claim of being scientific. Under the dual-level system, however, with stricter scrutiny for the "scientific," that near-scientific evidence would be excluded, while the clearly nonscientific might be admitted. The bifurcation of Rule 702 can mean the exclusion of better evidence than what the jury hears.

C. *Jurors' Perception of Scientific Evidence*

As indicated, the rationale for a specially strict scrutiny of scientific evidence as compared to other testimony seems to rest on the notion that jurors will give special deference to scientific evidence. Because science has a special aura for laypeople, it may (or will) be readily accepted by a jury. Jurors can analyze normal testimony, and courts need do little screening before jurors hear lay evidence. Jurors, on the other hand, have difficulty analyzing scientific testimony and will too readily accept it. Since they are likely to accede to it without analysis, courts should try to make sure that it is reliable before admitting scientific evidence.¹⁸

This argument, however, is not drawn from *Daubert's* logic. The Court did not hold that specially restrictive admissibility standards were required because jurors defer to science. The decision, once again, derived, purportedly, from the term "scientific knowledge," not from an assessment of the jury's abilities to evaluate science. If Rule 702 authorizes stricter screening for scientific evidence than for other expert testimony, it should somehow be found in the term "technical or specialized knowledge," not in assumptions about jurors aptitudes.¹⁹

18. *Developments in the Law—Confronting the New Challenges of Scientific Evidence*, 108 HARV. L. REV. 1481, 1524 (1995) ("The modern debate about the different classifications of expertise and their effect on judicial evaluation of expert testimony is largely the result of disagreement concerning the potential for particular types of testimony to sway jurors imprudently.").

19. *Daubert v. Merrell Dow Pharmaceuticals, Inc.*, 509 U.S. 579, 594-95 (1993). The Court, while not indicating that a concern over the juror's abilities to evaluate expert testimony was part of a Rule 702 consideration, did state

In any event, concern about the jurors' ability to evaluate science misses the point. The relevant comparison if the dual-level system is to be justified is not between the scientific and the lay witness, but between the scientific and the nonscientific expert. Do jurors really give more deference to an epidemiologist reporting a purportedly scientific study of the effects on human health of electromagnetic fields than to a treating physician who gives a personal, nonscientific opinion about causation or the likelihood of full recovery from injuries?²⁰ Is the DNA expert more deferred to than the accountant valuing business assets or the real estate appraiser?²¹ The notion that juries have special difficulties with scientific evidence in comparison to other expert testimony is hardly self-evident and has little support.²²

For example, at least one survey has found that jurors believe that they understand scientific evidence as well as other evidence.²³ Another study has found that jurors more willingly accept experts who present information nontechnically and give firm conclusions than those who do not.²⁴ If

that it could affect a Rule 403 analysis. *Id.* In doing so, *Daubert* did not distinguish scientific from other expert testimony. *Id.* "Expert evidence can be both powerful and quite misleading because of the difficulty in evaluating it. Because of this risk, the judge in weighing possible prejudice against probative force under Rule 403 of the present rules exercises more control over experts than over lay witnesses." *Id.* at 595.

20. Cf. SHEILA JASANOFF, SCIENCE AT THE BAR: LAW, SCIENCE, AND TECHNOLOGY IN AMERICA 124-26 (1995) (in discussing a toxic tort case, concluded, "The court, in other words, reaffirmed the jury's right to place treating physicians, who bore direct witness to [plaintiff's] condition, at the top of the hierarchy of credibility.").

21. Cf. *United States v. Starzecpyzel*, 880 F. Supp. 1027, 1048 (S.D.N.Y. 1995) ("With regard to scientific experts, a major rationale for *Frye*, and now *Daubert*, is that scientific testimony may carry an 'aura of infallibility.' . . . Skilled experts generally present less of a problem, as, with all due respect, accountants are unlikely bearers of an aura of infallibility." (internal citations omitted)).

22. See Champagne et al., *supra* note 6, at 388 ("[L]ittle is known, other than through anecdote, about how jurors are affected by expert testimony.").

23. Joseph L. Peterson et al., *The Uses and Effects of Forensic Science in the Adjudication of Felony Cases*, 32 J. FORENSIC SCI. 1730, 1748 (1987) ("Jurors indicated to us they believed they understood the scientific and physical evidence presented to them at least as well as, and commonly better than, other evidence in the case.").

24. Champagne et al., *supra* note 6, at 388. In one survey, 36% of jurors found experts most believable who presented information in a nontechnical manner, and 31% found most believable experts who reached firm conclusions. *Id.* The authors concluded, "Given the limits and uncertainties of scientific knowledge, firm conclusions are a sign that the [jury] should look upon the ex-

valid scientific knowledge is tentative, as *Daubert* recognized,²⁵ and often technical or probabilistic, the jury is less likely to believe the resulting scientific testimony than other kinds of expert opinions that are simply and definitely expressed.²⁶

While the tentative and technical nature of science may make jurors less deferential to it than other kinds of expertise, jurors can actually have better information for evaluating scientific than nonscientific expertise. Science should be based not on the word of a scientist, but data which is available to all.²⁷ Content, not personal authority,²⁸ should define good science.²⁹ At least theoretically jurors do not have to

pert with some distrust, yet the underlying reason for using experts—limited juror or judge knowledge of an issue—makes it understandable that jurors would like clearer answers to scientific and technical questions. It appears that the tentativeness of much scientific work is unappreciated by many jurors." *Id.*

25. *Daubert v. Merrell Dow Pharmaceuticals, Inc.*, 509 U.S. 579, 597 (1993) ("Scientific conclusions are subject to perpetual revision.").

26. See Randolph N. Jonakait, *Stories, Forensic Science, and Improved Verdicts*, 13 CARDOZO L. REV. 343, 347 (1991) (discussing how juries assimilate information into stories). Consequently, jurors should find more acceptable experts who present a complete story. See also Margaret B. Kovera et al., *Expert Testimony in Child Sexual Abuse Cases: Effects of Expert Evidence Type and Cross-Examination*, 18 LAW & HUM. BEHAV. 653, 668 (1994) (jurors' ratings of expert who presented probabilistic information was less positive than experts who presented more anecdotal information).

27. See ROBERT K. MERTON, *THE SOCIOLOGY OF SCIENCE: THEORETICAL AND EMPIRICAL INVESTIGATIONS* 273-74 (Norman W. Storer ed., 1973) ("The substantive findings of science are a product of social collaboration and are assigned to the community [There is] an imperative for the communication of findings. Secrecy is the antithesis of this norm").

28. Cf. BRUNO LATOUR, *SCIENCE IN ACTION: HOW TO FOLLOW SCIENTISTS AND ENGINEERS THROUGH SOCIETY* 31 (1987) ("Science is seen as the opposite of argument from authority."); KARL R. POPPER, *THE LOGIC OF SCIENTIFIC DISCOVERY* 46 (Torchbook ed., 1961) (No matter how intensely felt "a subjective experience, or a feeling of conviction, can never justify a scientific statement"); Alexander Morgan Capron, *Daubert and the Quest for Value-Free "Scientific Knowledge" in the Courtroom*, 30 U. RICH. L. REV. 85, 86 (1996) ("Science is oriented toward the truth but its claims are presented tentatively and are subject to refutation, with an emphasis on the quality of the data rather than on decisions produced by an hierarchical structure.").

29. Cf. MERTON, *supra* note 27, at 270 (Scientific claims are "subjected to preestablished impersonal criteria; consonant with observation and with previously confirmed knowledge. The acceptance or rejection of claims entering the lists of science is not to depend on the personal or social attributes of their protagonists"); STEVEN ROSE, *THE MAKING OF MEMORY: FROM MOLECULES TO MIND* 185 (Anchor ed., 1993) [hereinafter ROSE] ("This is what is meant by claiming that scientific knowledge is 'public' knowledge—that is, that it is in principle testable and verifiable by anyone/everyone and not merely a matter of private belief."); Murray Levine, *Scientific Method and the Adversary Model:*

just take a scientist's word for the presented conclusions, but can examine the grounds upon which it is based. Perhaps most important, that foundation can be examined and challenged by cross-examination and other experts.³⁰ As a result, jurors can have something to evaluate besides just the personal credibility of the testifying expert.

Much nonscientific expertise, however, is ultimately based upon personal authority leaving nothing to evaluate but credibility. This is especially so when an expert bases conclusions upon "experience" or "expertise." When an expert testifies that because of his training and experience that some fact is true, the expertise is really a claim of personal authority. In essence, such an expert testifies, believe what I say not because of the data I present, but because of who I am.

In such circumstances, the information and reasoning upon which the opinion is based is not accessible to all. The basis for the testimony is truly available only to the one person who has had that particular training and experience.³¹ As a result, neither cross-examination nor another expert can do much to elucidate, evaluate, or contest the data and premises upon which the testimony relies.³² The opponent can only attack the training, experience, or personal credibility of the expert or present another expert who, too, may base an opinion on training and experience. The jury, unlike with scientific expertise, has no way to assess the expert testimony

Some Preliminary Thoughts, AM. PSYCHOLOGIST 661, 664 (Sept. 1974) ("The canons of method require that we admit as scientific only that which is public and communicable. By those terms we mean that the observations may be made by any qualified observer who is in a position to observe and that we can tell the new observer how to put himself in such a position.").

30. MICHAEL J. SAKS & RICHARD VAN DUIZEND, *THE USE OF SCIENTIFIC EVIDENCE IN LITIGATION* 5 (1983) ("[T]he fact-finder need never take a scientific expert witness's 'word for it.'").

31. Cf. Richard A. Epstein, *Judicial Control Over Expert Testimony: Of Deference and Education*, 87 NW. U. L. REV. 1156, 1161 (1993) (When expert judgment is at stake "[i]t is often difficult for experts . . . to articulate the grounds on which their judgment rests. On biological or medical questions, for example, judgment often comes from an accretion of small bits of information into a whole that is more coherent than the sum of its parts.").

32. See Gross, *supra* note 6, at 1171-72 ("[A]n expert who testifies to conclusions that are based on her 'experience' and 'expertise' is less vulnerable to attack than one who relies on explicit data or specified lines of reasoning."); cf. Kovera, et al., *supra* note 26, at 669 (studying jurors' assessments of child abuse experts, the strength of a cross-examination had little effect on a jurors' judgments even though all the expertise was questionable).

other than by accepting or rejecting it based upon who the expert is and how she asserts her opinion.³³

This is not to suggest that juries necessarily evaluate scientific evidence well.³⁴ Even if they don't, the dual-level approach to expertise is justified only if jurors assess nonscientific expertise much better than the scientific kind. That proposition is supported by neither logic nor convincing data.

33. See Faigman, *supra* note 7, at 1085 (When conflicting empirical claims have not been adequately tested "jurors have no well-founded basis on which to choose between them. Such battles are likely to be won by the more persuasive witness, rather than the more persuasive facts or opinions."). See also Steven C. Bank & Norman G. Poythress, Jr., *The Elements of Persuasion in Expert Testimony*, 10 J. PSYCHIATRY & L. 173, 178 (1982) ("It is well documented by numerous laboratory studies that, other things being equal, [the expert witness] attributed to have the greater status or credentials is usually the more persuasive or influential."); cf. Morse, *supra* note 11, at 1026 ("[A] psychodynamic formulation [of behavior] is an unverifiable and unreliable causal account of an individual case Cross-examination will not be an effective tool for exposing the inaccuracy of psychodynamic formulations because the factfinders will have no means to resolve disputes.").

34. Even when the bases of scientific opinions are presented, some jurors are more likely to make personal judgments about the experts than the information relayed. Joseph Sanders, *From Science to Evidence: The Testimony on Causation in the Bendectin Cases*, 46 STAN. L. REV. 1, 38-39 (1993). Part of the problem is that even though scientific data is presented, cross-examination is often still an attack on credibility that does little to clarify the strengths and weaknesses of the opinion. *Id.* at 47. Cf. Bert Black et al., *Science and the Law in the Wake of Daubert: A New Search for Scientific Knowledge*, 72 TEX. L. REV. 715, 789 (1994) ("Veracity, memory, motivation, prejudices and biases—the weaknesses that cross-examination is best at ferreting out—are not very relevant to attacking an expert's reasoning . . ."). To improve our factfinding system, we should not only discuss the admissibility standards for expert testimony but also how expert evidence can best be presented and challenged so jurors are more likely to assess the information instead of just the personal credibility of the experts. Sanders, *supra* at 60-85.

The point remains, however, that often with nonscientific expertise the jurors will have nothing to evaluate but the personal credibility of the experts, and there is no reason to assume that jurors can evaluate nonscientific expertise better than the scientific kind. See Steven M. Egesdal, Note, *The Frye Doctrine and Relevancy Approach Controversy: An Empirical Evaluation*, 74 GEO. L.J. 1769, 1187-85 (1986) (reviewing studies on jurors' use of scientific evidence and concluding that jurors evaluate the science when they are made to understand the science, but evaluate the credibility of the experts when the science is not made understandable to the jury); cf. Ronald J. Allen & Joseph S. Miller, *The Common Law Theory of Experts: Deference or Education?*, 87 NW. U. L. REV. 1131, 1131 (1993) ("The deeper question is whether fact finders are to be educated by or to defer to experts. The various debates about expert testimony, as enlightening as they have been, cannot be resolved without addressing that question.").

D. *Expertise as a Social Construct*

Sociologists of science have contended that science has no magic spyglass on objective truth. Science, instead, presents social constructs, as other epistemological endeavors do.

The most significant insight that has emerged from sociological studies of science in the past 15 years or so is the view that science is *socially constructed*. . . . [T]he "facts" that scientists present to the rest of the world are not simple reflections of nature; that, these "facts" are produced by human agency, through the institutions and process of science, and hence they invariably contain a social component.³⁵

35. Sheila Jasanoff, *What Judges Should Know About the Sociology of Science*, 32 JURIMETRICS 345, 347 (1992). Elsewhere Jasanoff has summarized:

The authority of scientific claims derives, according to the sociological account, not directly, from the representation of physical reality, but indirectly, from the certification of claims through a multitude of informal, often invisible, negotiations among members of relevant disciplines. A complex network of people, methodologies, visual recordings or inscriptions and instruments (which themselves incorporate social conventions) must be brought into harmony in order to establish scientific claims as true [E]ven when a claim's factual status is still "in the making," its provisional nature may be screened from public view by a technique that sociologists of science call "boundary work": that is, a communally approved drawing of line between "good" and "bad" work (and, not trivially, between good and bad *workers*) within a single discipline, between different disciplines, and between "science" and other forms of authoritative knowledge.

JASANOFF, *supra* note 20, at 52-53. See also Margaret G. Farrell, *Daubert v. Merrell Dow Pharmaceuticals, Inc.: Epistemology (sic) and Legal Process*, 15 CARDOZO L. REV. 2183, 2196 (1994) [hereinafter Farrell] ("[S]ociologists of science have concluded that not only is the scientific process a social construct, but that scientific truths are socially constructed as well."); cf. Theodore R. Vallance, *Social Science and Social Policy: Amoral Methodology in a Matrix of Values*, 27 AM. PSYCHOLOGIST 107, 107 (Feb. 1972):

[S]cience is not value free, and . . . social science, whether 'purely theoretical' or 'applied,' is in its every phase entwined in the values of the society which or on which it would operate. The choosing of *what* to investigate in any field is culturally relevant and represents value judgments present in the culture and manifested in the scientist.

Steven Rose gives a good short history of this intellectual development. See ROSE, *supra* note 29, at 292-93. While Karl Popper saw science as the creation and testing of hypotheses with the hypotheses being abandoned when they were falsified, Thomas Kuhn contended that normal science "is not about testing hypotheses but merely about puzzle-solving within a given understanding of the world—an understanding that Kuhn called a paradigm." *Id.* (referring to THOMAS KUHN, *STRUCTURE OF SCIENTIFIC REVOLUTION* (1962)). Research that usually conflicts with a hypothesis is somehow made to fit the paradigm. *Id.* When the anomalies become too great, however, "revolutionary" science occurs with a new hypothesis generated; a paradigm shift then occurs. *Id.* "What

Consequently, "neither science nor law has a greater claim to the truth, and courts ought not to accept for legal, evidentiary purposes the conventional standards of validity and reliability established by scientists"³⁶ as *Daubert* seems to require. Courts ought to recognize that the admissibility standard has a "normative component" and, thus, "[c]onsideration of appropriate legal standards for the admission of scientific evidence is not, and should not be, divorced from the substantive contexts in which the standards are applied."³⁷

This analysis, while seeming to attack *Daubert* at its core, indicates yet again that Rule 702 should not be construed as having a dual-level approach to expertise. The fundamental conclusion is that science should not be seen as an objective purveyor of facts. Science is produced by a human agency and contains a social component. Science, consequently, does not just discover facts; it "constructs" them.

In saying this, the sociologists are not saying that science is distinctive. Instead, they are saying that science is like

made a paradigm attractive . . . ; and why did scientific revolutions occur? Kuhn left such questions as internal to science, . . . but the sociologists could see there was more to it than that, and the social order external to the laboratory might influence what went on inside. This enabled them to recapture the insights into the social functions and relations of science that an earlier generation of marxist scientists had already found, and it presaged their eventual move into a hyper-reflexive stratosphere with which scientific knowledge of the natural world itself dissolved into a miasma of social relations." *Id.* Perhaps most influential here has been Bruno Latour whose "deconstructionist accounts of science began with his experience as a post-doctoral anthropologist, when he spent a year as a partially participant observer in a California laboratory working on the identification and isolation of a neuro-hormone. The account he gave of this in his first book, and the theoretical extrapolations that have followed, have influenced a generation of sociologists of science." *Id.* at 293-94. See also BRUNO LATOUR, *SCIENCE IN ACTION* (1987); BRUNO LATOUR & STEVE WOOLGAR, *LABORATORY LIFE: THE CONSTRUCTION OF SCIENTIFIC FACTS* (1979).

36. Farrell, *supra* note 35, at 2198.

37. *Id.* at 2213. See also JASANOFF, *supra* note 20, at 44 & 209-10:

[S]cience in a legal setting is always bound up with some specific constructions of causation, blame, and responsibility [T]he [law's] choice between alternative scientific accounts necessarily involves normative, even political, judgments Instructing courts to take their cues from idealized stories of heroes and villains, such as the "good" scientific methodology of Gregor Mendel as opposed to the "bad" scientific practices of T. D. Lysenko, can therefore be deeply misleading. Other forms of guidance are needed, more realistically attuned to indeterminacy of scientific knowledge in the actual contexts of litigation, and mindful of the institutional strengths and weaknesses of judicial dispute resolution.

other fields of knowledge.³⁸ Nonscientific learning, as well as the scientific, is also mediated by human agency, and therefore, nonscientific fields of expertise the law draws upon must also be social constructs.³⁹ If scientists are not objective and disinterested purveyors of fact, other kinds of experts are not either.⁴⁰ And if most of the thoughts, conclusions, and conjectures by scientists can be wrong,⁴¹ then so too can be the conclusions of other experts. If science is a social construct, it is not fundamentally distinct from other kinds of knowledge, and the law should not treat it as fundamentally different from nonscientific expertise.

E. *One Fundamental Standard for all Expertise*

Scientific testimony should not be screened by a distinctly different, and more rigorous, standard than is used for nonscientific expertise. Rule 702's text, the difficulty and hazards of drawing the boundary line, the logic of *Daubert*, jurors' evaluation of expertise, the structure of science, and the sociology of science all indicate that the same fundamental standard ought to be used to assess all expertise. If so, even though *Daubert* was explicitly speaking about a specific form of expert testimony, the basic premises that support it should somehow be extended to all evidence offered under Rule 702. The articulation of such standards, however, is not

38. See LATOUR & WOOLGAR, *supra* note 35, at 31 ("[S]cientific activity is just one social arena in which knowledge is constructed.").

39. Cf. JASANOFF, *supra* note 20, at 207 ("There is no way for the law to access a domain of facts untouched by values or social interests.").

40. See David Faust, *Declarations Versus Investigations: The Case for the Special Reasoning Abilities and Capabilities of the Expert Witness in Psychology/Psychiatry*, 13 J. PSYCHIATRY & L. 33, 52 (1985) ("[A]s is true with all individuals, introspection about one's own judgment process and capacities can be very misleading. As many researchers have shown, individuals usually attend more closely to information that confirms their beliefs than information that does not."); cf. JAMES WATSON, *THE DOUBLE HELIX* 18 (1968) (stating that successful scientists recognize that many scientists are narrow-minded and stupid).

41. See W. I. B. BEVERIDGE, *THE ART OF SCIENTIFIC INVESTIGATION* 79-80 (Vintage ed., 1957):

Most [scientific] hypotheses prove to be wrong whatever their origin may be The productive research worker is usually one who is not afraid to venture and risk going astray, but who makes a rigorous test for error before reporting his findings [E]ven with men of genius, with whom the birth rate of hypotheses is very high, it only just manages to exceed the death rate.

an easy task: Any attempt to generalize *Daubert* beyond scientific proof runs into problems.

For example, *Daubert's* flexible framework may be so subject to different meanings that the opinion hardly gives useful guidance on the admissibility of scientific evidence.⁴² Certainly, the outpouring of comments as to what it means or ought to mean suggests that this is true.⁴³ If *Daubert* is not clear for resolving questions about scientific evidence, it does not seem to be able to give good guidance for other areas of expertise.

Indeed, even if its framework had a more determinate meaning, *Daubert* seems naively to have undertaken the impossible by creating one set of guidelines for all of science when good science uses more than one method.⁴⁴ And of course, if one set of guidelines, no matter how clear, cannot

42. See Faigman, *supra* note 10, at 555 ("*Daubert* provides precious little advice on how to successfully resolve the tangled relations between law and science."); see also Randolph N. Jonakait, *The Meaning of Daubert and What That Means for Forensic Science*, 15 CARDOZO L. REV. 2103, 2104 (1994) ("The wonder of flexible inquiries is that they can so often be made to fit just about any predetermined conclusion The skeptic may feel that flexible standards seldom produce results; instead, they are devices merely used to justify conclusions reached on other grounds.")

43. See Faigman, *supra* note 10, at 555 n.3 ("The lack of advice to be found in *Daubert* perhaps is best illustrated by the surplus of commentary seeking to interpret and advance the *Daubert* lesson" and collecting a representative sample of the commentary.)

44. See, e.g., Stephen J. Ceci & Urie Bronfenbrenner, *On the Demise of Everyday Memory*, 46 AM. PSYCHOLOGIST 27, 28 (1991) [hereinafter Ceci Bronfenbrenner] ("There is nothing in the classical conception of science nor in the modern sociological view of it . . . that would support equating science with a specific set of methods."); R. C. LEWONTIN, *Facts and the Factitious in Natural Sciences in Questions of Evidence: Proof, Practice, and Persuasion Across the Disciplines* 478, 489 (James Chandler et al. eds., 1994) [hereinafter Lewontin] ("The demands for rigor of experimental design in theoretical inference vary widely in science from field to field, sometimes between very closely allied domains off research."); Peter Schuck, *Multi-Culturalism Redux: Science, Law, and Politics*, 11 YALE L. & POL'Y REV. 1, 20 (1993) ("[T]he conventional contrast between 'hard' and 'soft' sciences tracks important divisions that belie the merely superficial unity of the scientific culture Theoretical and experimental scientists do quite different things and think in different ways."); Paul S. Milich, *Controversial Science in the Courtroom: Daubert and the Law's Hubris*, 43 EMORY L.J. 913, 920 (1994) ("But 'the scientific method' is not the same for all disciplines of science and in any case does not specify a particular level of 'appropriate validation' before a principle or methodology is accepted as reliable."). Cf. Black et al., *supra* note 34, at 747 n.226 ("Though the [*Daubert*] Court referred to the scientific method, its opinion is generally sensitive to the fact that science cannot be reduced to a single method."); Farrell, *supra* note 35, at 2203 ("[T]o provide uniformity, the courts would have to establish a body of precedent with regard to

truly evaluate all of science, it seems silly to extend that framework to the myriad other forms of expert evidence.⁴⁵ Certainly specific components of *Daubert's* flexible framework such as falsifiability, peer review, and error rates seem impossible to apply to such expert witnesses as the mechanic, the accountant, the real estate appraiser, the harbor pilot, and perhaps even the psychologist.⁴⁶

Even so, however, as Part II discusses, some basic principles do underlie *Daubert*, and these principles can and should be extended to expertise more generally.

valid scientific methodologies in various disciplines, in effect legitimating particular scientific paradigms.”).

45. One study of experts in state civil cases found that:

Half of the experts in our data were medical doctors, and an additional 9% were other medical professionals—clinical psychologists, rehabilitation specialists, dentists, etc. Engineers, scientists and related experts made up the next largest category, nearly 20% of the total. The only other sizeable categories were experts on various aspects of business and finance (11%), and experts in reconstruction and investigation (8%).

Gross, *supra* note 6, at 1119. Another survey found that “[t]he plurality (48%) were physicians. The other areas of expertise were wide-ranging. There were four veterinarians and a smattering of psychologists, accountants, economists, historians, political scientists, sociologists, lawyers, personnel specialists, biochemists, and engineers [T]here were almost as many areas of expertise as there were experts” Champagne et al., *supra* note 6, at 381. Cf. CARNEGIE COMMISSION ON SCIENCE, TECH., AND GOV'T, SCIENCE AND TECH. IN JUDICIAL DECISION MAKING: CREATING OPPORTUNITIES AND MEETING CHALLENGES 22 (1993) (“Engineers are among the most frequently encountered expert witnesses; they appear in a wide variety of cases such as construction disputes, product liability actions, and complex environmental litigation.”); Epstein, *supra* note 31, at 1159 (“The amount of variation in the cases covered by Rules 702 and 703 is enormous, and it is highly unlikely that any uniform approach will not suffer from possible cross-pressures and counterexamples.”).

46. Cf. Edward J. Imwinkelried, *The Next Step After Daubert: Developing a Similarly Epistemological Approach to Ensuring the Reliability of Nonscientific Expert Testimony*, 15 CARDOZO L. REV. 2272, 2285 (1994) (“[A]lthough the *Daubert* Court may have selected the optimal test for the admissibility of scientific evidence, that test is useless as a criterion for the admissibility of other types of expert evidence.”); James T. Richardson et al., *The Problems of Applying Daubert to Psychological Syndrome Evidence*, 79 JUDICATURE 10, 11 (1995) (“[T]he nature of certain social and behavioral science theories may be inherently inconsistent with *Daubert* criteria such as ‘falsifiability’ and ‘error rates.’”).

PART II

A. *Science Works*

The view of science as a social construct has power, but the law rejects the extremist consequences of the sociological claim. Taken to its limits, this belief would see all scientific evidence as equal. If it is merely socially constructed, none should be "privileged" over others. Since it is all of equal status, all of it should be admitted. On the other hand, if no scientific evidence is more worthy or convincing or right than others, then admission of such testimony would not truly aid the jury.⁴⁷ Indeed, if all social constructs are equal, the jury, unaided, is as capable of producing a construct as valid as it would with expert help.

Evidence law as interpreted by *Daubert* implicitly, but definitively, rejects these radical views. All science is not admitted; all is not excluded. Science is not all equal. For the purpose of our trials, some scientific evidence is to be trusted more than others.

This is correct. The logic of trials is that of ordinary common sense,⁴⁸ and people using common sense do not see scientific principles as merely equal social constructs. The scientific and technological principles that indicate my car will stop when I step on the brakes are facts that I have come to believe reflect the world. If there is social construction in science, we make distinctions among the constructs. I have my daughter vaccinated, but I do not ask the doctor to bleed her. Although my common sense may not be able to articulate the reasons, I believe that there are valid bases for making such distinctions. When we get in an airplane or elevator, or take an antibiotic, or turn on a computer or television, we have

47. The widespread admission of expert evidence, however, will make trials more costly. See Ronald J. Allen, *Expertise and the Daubert Decision*, 84 J. CRIM. L. & CRIMINOLOGY 1157, 1162 (1994) (The liberal admission of expert testimony "would tend to make cases involving expertise more protracted."). Cf. Farrell, *supra* note 35, at 2215 (The liberal admission of scientific evidence "would make courts, particularly juries, the instruments of wealth redistribution rather than more representative bodies.").

48. See GRAHAM C. LILLEY, AN INTRODUCTION TO THE LAW OF EVIDENCE 27 (2d ed. 1987) ("[T]he test of probative value is derived from commonplace experience. That is, the test usually involves no more than a commonsense determination, made in the light of human observation and experience, that certain events or conditions either are causally connected or normally associated with other events or conditions.").

confirmation in our daily lives that some science is truly reliable and not just a mere social construct.⁴⁹

Science, however, is a product of its time. Scientists are not infallible and without biases.⁵⁰ No sensible defender of science would maintain that scientists are truly objective, but that does not mean that human and social limitations affect all of science identically. As Philip Kitcher states, "[O]f course people make mistakes. Our observational and inferential procedures for generating belief are fallible. They are not *equally* fallible."⁵¹ Furthermore, sensible scientists would not claim that science has found absolute truth about the empirical world. They might claim, however, with good grounds, that in a universe where perceptions and beliefs are imperfect, science has been the best human method for advancing understanding of the natural world.

Scientists are not infallible. Science is not a process by which we go from no knowledge to some knowledge, or from some knowledge to total knowledge. Rather it is a process by which scientists go from some knowledge to more knowledge. The important feature of science is not that it *always* produces increased knowledge but that *sometimes* it does. Science is not a perfect machine for grinding out true claims about the world in which we live, but it is the best of all the imperfect machines developed to date.⁵²

49. See Steven Weinberg, *The Methods of Science . . . And Those by Which We Live*, 8 ACAD. QUESTIONS 7, 12 (Spring 1995) ("It is a fallacy . . . that, because the [scientific] process is a social one, the end product is a mere social construct."). See also LATOUR & WOOLGAR, *supra* note 35, at 127 ("[T]o say that [Thyrotropin Releasing Factor] is constructed is not to deny its solidity as a fact."); cf. Johannes F. Nijboer, *Forensic Expertise in Dutch Criminal Procedure*, 14 CARDOZO L. REV. 165, 186 (1992) [hereinafter Nijboer] ("Although the prevailing scientific-sociological opinion holds that factual knowledge is a social construct, factual knowledge is not a random social construct.").

50. See DAVID L. HULL, *SCIENCE AS A PROCESS: AN EVOLUTIONARY ACCOUNT OF THE SOCIAL AND CONCEPTUAL DEVELOPMENT OF SCIENCE* 22 (1988) ("[I]ndividual bias is extremely difficult to eliminate.").

51. PHILIP KITCHER, *THE ADVANCEMENT OF SCIENCE: SCIENCE WITHOUT LEGEND, OBJECTIVITY WITHOUT ILLUSIONS* 139 (1993).

52. HULL, *supra* note 50, at 26 (1988). See also KITCHER, *supra* note 51, at 161 ("That thesis does not imply that we have unbiased access to nature, merely that the biases are not so powerful that they prevent us from working our way out of false belief."); cf. Weinberg, *supra* note 49, at 9 ("I do not see why the fact that we are discovering not only the laws of nature in detail, but what kinds of laws are worth discovering, should mean that we are not making objective progress.").

Sociologists may preach social constructions, but scientists see progress or the lack of it towards understanding the empirical world.⁵³ Scientists see good or bad science; they see knowledge advancing or standing still. We, in our daily lives, often without reflecting on it, accept and reject views of the world based on what science has learned. Common experience has us side with the scientists. Science works not perfectly, but it works.⁵⁴ That common sensical view is the one accepted by *Daubert* when it concluded that judges must actively screen scientific evidence.⁵⁵ The important question then, as all concede, is how, if scientists are not objective, disinterested generators of information, but, being human, are biased and prey to social forces and limitations,⁵⁶ is it possi-

53. Cf. HULL, *supra* note 50, at 26 ("Although scientists themselves are frequently uneasy about using terms like 'true' and 'objective,' all the time that they spend running experiments and making extensive and careful observations is inexplicable on the assumption that knowledge is in any significant sense socially determined."). See also LATOUR & WOOLGAR, *supra* note 35, at 175 ("[P]racticing scientists . . . are unlikely to adopt this perspective [of social construction] for very long before returning to the notion that facts exist, and that it is their existence that requires skillful revelation.").

54. See Charles Kester, *The Language of the Law, the Sociology of Science and the Troubles of Translation: Defining the Proper Role for Scientific Evidence of Causation*, 74 NEB. L. REV. 529, 563 (1995) ("Put differently, science works. More importantly, the American public believes that science works."); cf. Wendy E. Wagner, *The Science Charade in Toxic Risk Regulation*, 95 COLUM. L. REV. 1613, 1619 n.21 (1995) ("Despite this lack of an underlying 'truth' in scientific conclusions, there is virtual unanimity that the scientific process does assist in determining truth by sorting out some 'falsehoods' through testing and through the replication of tests.").

55. See Faigman, *supra* note 10, at 555 ("The single most important 'guide-post' contained in *Daubert* is the Court's directive to judges to actively evaluate scientific evidence."). Sheila Jasanoff, a prominent sociologist of science, does not contend that scientific evidence should go unfiltered, but only that the power should be carefully used. Janasoff, *supra* note 35, at 359. She concludes that when judges exclude such evidence,

[t]hey help shape an image of reality that is colored in part by their own preferences and prejudices about how the world should work. Such power need not always be held in check, but it should be exercised sparingly. Otherwise, one risks substituting the expert authority of the black robe and the bench for that of the white lab coat—an outcome that poorly serves the causes of justice or of science.

Id.

56. See, e.g., BEVERIDGE, *supra* note 41, at 66-67:

It is not at all rare for investigators to adhere to their broken hypotheses, turning a blind eye to contrary evidence, and not altogether unknown for them deliberately to suppress contrary results A danger constantly to be guarded against is that as soon as one formulates an hypothesis, parental affection tends to influence observations, inter-

ble for science to move from some knowledge to more knowledge? When is science to be regarded above social construction and reliable enough to be admitted into our trials? While science may use many tools and methods, science that gives the most confidence for truly advancing knowledge does rest on some fundamental principles.⁵⁷

B. *Encounters With Nature*

Good science consists of a series of mechanisms that reduce the chances that biases, idiosyncrasies, faulty logic, and misperceptions will lead to a scientist's self-deception.⁵⁸ The most basic of these is that we truly learn about the world not

pretation and judgment; "wishful thinking" is likely to start unconsciously.

See also Elizabeth Howard, Note, *Science Misconduct and Due Process: A Case of Process Due*, 45 HASTINGS L.J. 309, 320 (1994) ("[T]he possibility of self-deception is always present in science research.").

57. Scientists believe that while methodological rules differ across scientific fields, common principles for the assessment of science exist. See, e.g., KITCHER, *supra* note 51, at 85-86:

[A]t different stages in the history of science, different individual scientists have expressed different views on matters methodological. Does it follow from this that there is no set of methodological rules that allies to all sciences at all times? No. If we think of a set of methodological rules as formulating the optimal ways for scientists to form conclusions in certain contexts, then the mere fact that people have sometimes disagreed on what is best does not show that there is no optimum.

See also *id.* at 5 ("[W]hile there is no systematic way to generate new hypotheses, once hypotheses have been proposed there are principles for their proper assessment in light of statements of evidence."); BEVERIDGE, *supra* note 41 at xi ("[T]here are some basic principles and mental techniques that are commonly used in most types of [scientific] investigation . . ."); cf. Nancy Levit, *Listening to Tribal Legends: An Essay on Law and the Scientific Method*, 58 FORDHAM L. REV. 263, 265 (1989) ("The principles of scientific inquiry are essentially criteria of rationality."); Bert Black, *A Unified Theory of Scientific Evidence*, 56 FORDHAM L. REV. 595, 622 (1988) ("While there is no universal algorithm for verifying scientific validity, this does not mean that no basis exists for judging theories and claims to scientific knowledge.").

58. See Vallance, *supra* note 35, at 107 ("The methodology of science is relatively, but only relatively, value free . . . [Scientific] methodology is a set of rules to keep the practitioner of science from misleading himself about what he is observing and what he is concluding in his study of natural events."). See also BEVERIDGE, *supra* note 41, at 68 ("Unless observations and experiments are carried out with safeguards ensuring objectivity, the results may unconsciously be biased."); cf. SAKS & VAN DUIZEND, *supra* note 30, at 5 (1983) (Scientific evidence's "advantage over the testimony of lay witnesses is that scientific evidence at its best is not subject to the limitations of human perception, memory, bias, or interest.").

from authorities, not from hypothesis-building, not from logic, but by studying the world.⁵⁹ Science is derived from a rigorous, critical observation of nature. In this process, insights, speculations, informed judgment, and theory-formation are important, but they are not goals in themselves. They only serve to guide the scientist on how to encounter nature so as to learn from it.⁶⁰ While historical, social, or personal prejudices, misconceptions, or concerns may have helped produce the hypothesis, the theory can be accepted only if it somehow withstands encounters with nature.⁶¹

That scientists must go outside themselves and rendezvous with the world acts as a limitation on individual and social bias. This does not mean, however, that every scientist will see nature similarly. Scientists confront creation in different ways, and their disparate encounters inevitably lead to

59. See HERBERT BUTTERFIELD, *THE ORIGINS OF MODERN SCIENCE* 91 (Free Press ed., 1965) (Before modern science, "[w]hen there was anything that needed to be explained those men would not elicit their theories from the observations themselves—they would still draw on that whole system of explanation which had been provided for them by the ancient philosophy. Sir Francis Bacon, early in the seventeenth century, complained of this divorce between observation and explanation, and it was part of his purpose to show how the latter ought to arise out of the former."). See also *id.* at 51 ("In the later middle ages men realised that in the last resort everything depended on observation and experience, on dissection and experiment . . ."); Lee Loevinger, *Standards of Proof in Science and Law*, 32 *JURIMETRICS* 323, 324 (1992) ("Bacon argued that science involves two different mental processes, the making of a discovery and the demonstration of its truth, and that direct observation of nature is the only path to truth. This reliance on empirical observation is still the fundamental premise of science.").

60. See BEVERIDGE, *supra* note 41, at 63 ("[A hypothesis'] main function is to suggest new experiments or new observations. . . . Another function is to help one see the significance of an object or event that otherwise would mean nothing. . . . Hypotheses should be used as tools to uncover new facts rather than as ends in themselves.").

61. See KITCHER, *supra* note 51, at 306 ("[W]e work our way free of the mistakes of earlier [scientific] generations through further encounters with nature."). See also HULL, *supra* note 50, at 28 ("The truth of empirical claims, including laws of nature, are tested by what in fact does happen."); Task Force of the Presidential Advisory Group on Anticipated Advances in Science and Technology, *The Science Court Experiment: An Interim Report*, 193 *SCIENCE* 653, 656 n.1 (1976) ("We use the expression 'scientific fact' to mean a result, or more frequently the anticipated result, of an experiment or an observation of nature."); cf. THOMAS KUHN, *STRUCTURE OF SCIENTIFIC REVOLUTION* 77 (1962) ("An accepted theory or paradigm gets rejected not just by comparison with nature, but only with the simultaneous acceptance of another. This requires the comparison of both paradigms with nature and with each other."). But see LATOUR & WOOLGAR, *supra* note 35, at 243 ("Scientific activity is not 'about nature'; it is a fierce fight to construct reality.").

differences among them.⁶² Moreover, as the sociologists maintain, these experiences are not value-free. The investigator brings to them a belief-system that affects what is perceived.⁶³

As a result, science has learned that not all encounters with nature are equal. The most valuable ones are those that test hypotheses.⁶⁴ Until testing is done and data exists, a scientific claim, conclusion, theory, or pronouncement can be nothing more than the assertion of personal authority.⁶⁵ Unlike some other forms of learning, the scientist is not at an end, but a starting point, when apparent knowledge has been gained from logic or experience. Such an intuition is merely the preliminary phase to the sometimes difficult and often

62. See KITCHER, *supra* note 51, at 163:

Social forces might be so strong at the level of individual practice that precisely the same changes in individual practice would occur whatever stimuli impinged upon the individual scientist. This formulation is surely false. Scientists who are apparently subjected to the same social forces disagree because they have engaged in different encounters with nature.

See also *id.* at 97 ("Kuhn and Feyerabend achieved the important insight that different communities of scientists, working in the same field, may organize the aspects of nature that concern them in different ways.").

63. Cf. Stephen A. Newman, *Assessing the Quality of Expert Testimony in Cases Involving Children*, 22 J. PSYCHIATRY & L. 181, 207 (1994):

At some point in the course of the expert's inquiry, she will reach some conclusions, even if tentative, about key issues in the case. After that point, the expert's *perceptions* may be affected by those conclusions. It is a commonplace observation that one sees what one expects to see and screens out the rest.

Cf. also Levine, *supra* note 29, at 665 (explaining that in psychology, the observer influences and is influenced; he changes what he measures). But cf. HULL, *supra* note 50, at 481 ("Just because all scientific theories are underdetermined by anything that might be called 'the facts,' it does not follow that evidence is irrelevant.").

64. Cf. Imwinkelried, *supra* note 46, at 2277 ("In effect, Justice Blackmun [in *Daubert*] posed the epistemological question: *How* does a scientist come to know that a proposition is true? He looked to the methodology of Newtonian experimental science to answer the question; the process of developing and testing hypotheses explains how a scientist does so." (emphasis added)).

65. See, e.g., KITCHER, *supra* note 51, at 33 ("[Darwinian] biologists are compelled to advance hypotheses about the historical development of life, and it is incumbent on them to specify ways of testing these hypotheses (and hence to undertake [such testing]), if they are to avoid the charge that evolutionary biology is simply an exercise in fantasizing."). See also HULL, *supra* note 50, at 278 ("Theories that explain phenomena are necessary to raise them above the level of curiosities, but theories also need data if they are to be taken seriously.").

creative process of testing.⁶⁶ The scientist with the apparently innovative insight may be important, but the one who can figure out how to test it in nature may be more so.⁶⁷

C. *Proof by Disproof*

In one view of science, that testing is of a particular sort, attempts at falsification,⁶⁸ and falsifiability is at the contro-

66. See POPPER, *supra* note 28, at 27 ("A scientist . . . puts forward statements, or systems of statements, and tests them step by step. In the field of empirical sciences, more particularly, he constructs hypotheses, or systems of theories, and tests them against experience by observation and experiment."); LEWONTIN, *supra* note 44, at 480:

These postulations [about causation and the necessary interconnection between repeatable phenomena] demand the gathering of facts: observations from nature or from the deliberate perturbations of nature that are called experiments. When the facts are in, they can be compared with the postulated relations to confirm or falsify the hypothetical world."

See also Black et al., *supra* note 34, at 757 ("No matter how clever and brilliant a hypothesis might be, it must undergo corroboration through critical examination and empirical testing. Put more simply, science involves both the acquisition of knowledge and the justification of knowledge."); Faigman, *supra* note 10, at 570 ("A basic tenet of the scientific method is that hypotheses will be rigorously tested, however, promising the early findings.").

67. See, e.g., ANTONIO R. DAMASIO, DESCARTES' ERROR: EMOTION, REASON, AND THE HUMAN BRAIN xii (1992). The author noticed that a specific brain lesion caused both flawed reasoning and impaired feelings and hypothesized "that feeling was an integral component of the machinery of reason. Two decades of clinical and experimental work with a large number of neurological patients have allowed me to replicate this observation many times, and to turn a clue into a testable hypothesis." *Id.* The author also discusses testing of the hypothesis, including comparisons of skin conductance responses to disturbing images between people with frontal lobe damage with those without such damage. *Id.* at 205-22.

See also EDWARD O. WILSON, NATURALIST (Warner Books ed., 1994). The author, as a biogeographer, "had conjured a plausible image of the dynamic equilibrium of species, with new colonists balancing the old residents that become extinct, but . . . could offer very little direct evidence." *Id.* at 260. He then tested his hypothesis by fumigating some Florida Keys to see how they became repopulated and concluded, "[a]t least the cruder predictions of the theory of island biogeography had been met." *Id.* at 280.

68. See POPPER, *supra* note 28, at 42 ("[W]hat characterizes the empirical method is its manner of exposing to falsification, in every conceivable way, the system to be tested. Its aim is not to save the lives of untenable systems but, on the contrary, to select the one which is by comparison the fittest, by exposing them all to the fiercest struggle for survival."); see also Black et al., *supra* note 34, at 753-54 ("[T]he single most salient characteristic is falsifiability. For scientists, a new idea or explanation is not valid unless there is the possibility that empirical testing can prove it false and until it has withstood thoughtful efforts at falsification.").

versial heart of *Daubert's* notion of science.⁶⁹ Falsifiability is demanded because in an inductive world a proposition can never be definitively proved true. We cannot establish that all swans are white by parading ten, one hundred, or one thousand white swans, for the next swan might still be black, and the postulate disproved.⁷⁰ The scientist, instead, should seek the only definitive proof; that which would prove the thesis wrong.⁷¹ He should attempt to find black swans. If, however, rigorous attempts at falsification fail to prove the hypothesis wrong, the scientist has reasons to think that the conjecture might be right. Still, the acceptance of the hypothesis must remain provisional because the next attempt at falsification may succeed.⁷²

A philosopher may so define science, but scientists do not consistently operate in this manner. Scientific investigators do not just seek to prove their ideas wrong to gain provisional

69. See *Daubert v. Merrell Dow Pharmaceuticals, Inc.*, 509 U.S. 579, 593 (1993) (noting that testing and falsifiability are the distinguishing factors of science). Compare *Daubert*, 509 U.S. at 600 (Rehnquist, C.J., concurring) ("I am at a loss to know what is meant when it is said that the scientific status of a theory depends on its 'falsifiability,' and I suspect some [federal judges] will be, too.") with John I. Thornton, *Courts of Law v. Courts of Science: A Forensic Scientist's Reaction to Daubert*, 1 SHEPARD'S EXPERT & SCI. EVIDENCE Q. 475, 478 (1994) ("Rehnquist apparently did not read Popper, because Popper's explanation of what he meant [by falsifiability] are palaces of clarity.").

But cf. Allen, *supra* note 47, at 1171-72 (questioning whether the falsifiability principle captures many aspects of science and concludes that it "excludes a substantial portion of what is presently admitted as expert testimony."); Sean O'Connor, *The Supreme Court's Philosophy of Science: Will the Real Karl Popper Please Stand Up?*, 35 JURIMETRICS J. 263, 263 n.3 (1995) (claiming that Popper's falsifiability criterion was meant to separate "scientific and empirical knowledge from other matters," not good science from the bad).

70. See POPPER, *supra* note 28, at 40-41 ("Theories are . . . never verifiable These considerations suggest that not the verifiability but the falsifiability of a system is to be taken as a criterion of demarcation [I]t must be possible for an empirical scientific system to be refuted by experience." (emphasis added)).

71. Cf. POPPER, *supra* note 28, at 50 (stating that strict proof or disproof is never possible because of possibility that experimental results were unreliable).

72. See POPPER, *supra* note 28, at 33.

[A] positive decision can only temporarily support the theory, for subsequent negative decisions may always overthrow it. So long as a theory withstands detailed and severe tests and is not superseded by another theory in the course of scientific progress, we may say that it has 'proved its mettle' or that it is "corroborated."

(Emphasis added). See also Faigman, *supra* note 10, at 1018 ("[T]he merit of a scientific statement depends on the degree to which it has survived attempts at falsification." (emphasis added)).

knowledge.⁷³ They also seek confirmation of what they think they already know.⁷⁴

Even if, however, falsification does not define or describe how good scientists should, ought, or must always operate, it does give an important insight. Science does not proceed by confirmations alone.⁷⁵ Experience has taught that scientists can too readily accept personal theories.⁷⁶ Scientists, like the rest of us, want what they believe to be actually true. Humans tend to see what they want to see, and the scientist seeking confirmation for a treasured conjecture is likely to find it. Experience has taught that a scientist will naively believe that a conclusion is justified if only affirmation for it is sought.⁷⁷ Indeed, the sensible scientist quickly learns that before he should even believe his own postulates, much less convince others of them, he ought to seek what his theory says should be forbidden.⁷⁸

Although many techniques can be found in science, at its core is this method—proof by disproof.⁷⁹ This may not be all that a scientist does and proof by disproof does not guarantee

73. See Allen, *supra* note 47, at 1171 ("Scientists do not believe that all they know are negatives; they believe they know a lot of positive truths, and are learning more every year. Most scientific work is designed to expand the reach of scientific theories, to extend them into new domains, rather than to falsify them.").

74. Cf. STEPHEN JAY GOULD, *HEN'S TEETH AND HORSES TOES* 255 (1983) ("In science, 'fact' can only mean 'confirmed to such a degree that it would be perverse to withhold provisional assent.'").

75. Cf. LEWONTIN, *supra* note 44, at 479:

Most natural scientists . . . are really positivists. They rely heavily both on confirmation and falsification, and they believe that the gathering of facts, followed by inference rather than the testing of theories, is the primary enterprise of science. At times they speak of "strong inference," by which they mean something close to a Popperian falsification criterion

76. See BEVERIDGE, *supra* note 41, at 148 ("Nothing could be more damaging to science than the abandonment of the critical attitude and its replacement by too ready acceptance of hypotheses put forward on slender evidence. The inexperienced scientist often errs in being too willing to believe plausible ideas.").

77. Cf. BEVERIDGE, *supra* note 41, at 74 ("Probably the main characteristic of the trained thinker is that he does not jump to conclusions on insufficient evidence as the untrained man is inclined to do.").

78. See Thornton, *supra* note 69, at 479-80 (stating that a good scientific theory forbids certain things to happen, and every genuine test of a theory is an attempt to refute the theory).

79. Ceci & Bronfenbrenner, *supra* note 44, at 28 ("[A]t its core, science is a strategy of 'proof by disproof.' Scientists seek to assess the validity of a proposition by disconfirming alternative explanations.").

that what survives is reliable, but science has learned that proof only by confirmation is too likely to be a product of bias, misperception, or wishful thinking to be trusted. Proof by confirmation alone is too likely to be a social construct to be reliable.

D. *Rigorous Testing*

Individual prejudice is potentially so strong that merely seeking what should not occur is insufficient to overcome possible bias. The search must be done in a rigorous fashion, and that normally requires controls.⁸⁰

Without controls, the cause of an outcome cannot be determined. As E. Bright Wilson said, "If one doubts the necessity for controls, reflect on the statement: 'It has been conclusively demonstrated by hundreds of experiments that the beating of tom-toms will restore the sun after an eclipse.'"⁸¹ The proper methodology of controlled experiments, tests, or inquiries may vary from field to scientific field, but without

80. See BEVERIDGE, *supra* note 41, at 20:

The "controlled experiment" is one of the most important concepts in biological experimentation. In this there are two or more similar groups . . . : one, the "control" group, is held as a standard for comparison, while the other, the "test" group, is subjected to some procedure whose effect one wishes to determine. The groups are usually formed by "randomisation," that is to say, by assigning individuals to one group or the other by drawing lots or by some other means that does not involve human discrimination. The traditional method of experimentation is to have the groups as similar as possible in all respects except in the one variable factor under investigation, and to keep the experiment as simple as possible.

Cf. John Monahan & Laurens Walker, *Social Authority: Obtaining, Evaluating, and Establishing Social Science in Law*, 134 U. PA. L. REV. 477, 502 (1986):

There is consensus among social scientists of all disciplines that research must possess "validity." That is, the methods used in research must be able to justify the conclusions drawn by the investigator To have "high" validity, a study must rule out, or "control for," competing hypotheses that may account for an observed state of affairs.

See also GARY TAUBES, *BAD SCIENCE: THE SHORT LIFE AND WEIRD TIMES OF COLD FUSION* 120-21 (1993):

This is the most fundamental commandment in the canon of experimental technique. To reach an unimpeachable conclusion establishing the cause of an effect, run controls. E. Bright Wilson, Jr., in his classic 1952 volume *An Introduction to Scientific Research*, described controls as "similar test specimens which are subjected to as nearly as possible the same treatment as the objects of the experiment, except for the change in the variable under study."

81. TAUBES, *supra* note 80, at 162.

controls, science has learned, a conclusion is too likely to be a construct to be trusted.⁸²

E. *Biased Individuals; Objective Communities*

Good scientists do try to reduce the chances of self-deception by seeking disproof through rigorous, usually controlled, encounters with nature,⁸³ and we may be able to rely most firmly on a result when a scientist has refuted a favorite hypothesis.⁸⁴ Science as a system, however, does not depend on scientists proving their own ideas wrong or being able to

82. Cf. BEVERIDGE, *supra* note 41, at 25 ("Unless the basic needs of the controlled experiment can be satisfied it is better to abandon the attempt Most of the experiments have proved nothing [when] the controls were not strictly comparable."). See also Robert M. Andersen, *The Federal Government's Role in Regulating Misconduct in Scientific and Technological Research*, 3 J.L. & TECH. 121, 128-29 (1988):

Scientists are subject to self-deception or forms of experimental bias that they cannot reasonably be expected to control Experimental bias in studies using human subjects is so common that the use of a "double blind" has become standard practice not only in human research but also in animal experimentation. A double blind prevents both the experimenter and the subjects of the experiment from knowing which subjects are the controls.

83. See Faust, *supra* note 40, at 53:

In branches or areas of science in which accurate judgments are possible, . . . this success is not based on some unusual judgment power on the part of the practitioners. Rather, success is primarily based on the availability of a well-developed science that provides established theory, precise measurement techniques, and prespecified procedures and judgment guidelines.

Cf. Douglas R.O. Morrison, *The Rise and Fall of the 17-keV Neutrino*, 366 NATURE 29, 32 (1993):

This over-enthusiasm [for the desired result] and the selection of only positive experiments, is reminiscent of the Anglo-Saxon legal method, . . . [where] two lawyers oppose one another; each presents the argument favourable to his side and, where possible, criticizes the other sides' arguments. Good arguments by the opposition are ignored. In the scientific method, each individual is expected to consider all the arguments, theories and facts and to evaluate them critically to obtain a consistent conclusion, even though it may require one to reject some theories and/or experiments.

84. See HULL, *supra* note 50, at 394 ("When scientists refute their own favorite hypotheses or their opponents confirm them, one can place considerable confidence in the results."); cf. LATOUR & WOOLGAR, *supra* note 35, at 116 (quoting R. Guillemin, an eminent pharmacologist, from an interview with Latour, who similarly commented that "the only thing I can believe in this field are the retractions.").

transcend their inherent limitations.⁸⁵ Instead, science produces something more than mere social constructs primarily because it is a social activity. Scientists want to influence others and want credit for what they have produced, as sociologists and scientists recognize.⁸⁶ Science is a community enterprise, but instead of being a detriment, it is this community as a whole that actually helps produce disinterested knowledge.

This is not, however, because of schoolbook notions of one scientist impartially reproducing the work of another.⁸⁷ Few scientific rewards come from merely testing or replicating the work of others,⁸⁸ and too much science is produced for it all to be rigorously probed.⁸⁹ While it may be true that "[t]he most important testing that occurs in science is one scientist testing the views of another,"⁹⁰ that seldom occurs.⁹¹ A scientist

85. See HULL, *supra* note 50, at 435 ("[A] scientist can in principle subject his or her own views to testing, but if science had to depend solely on individual scientists proving their own ideas wrong, it would be in real trouble.").

86. See HULL, *supra* note 50, at 305 (emphasis added):

As multifarious as science has been and continues to be, a great deal about it can be explained by reference to just three elements: a desire to understand the world in which we live, the allocation of responsibility for one's contributions (both credit and blame), and the mutual checking of these contributions: in short, *curiosity*, *credit*, and *checking*.

87. See JASANOFF, *supra* note 20, at 214 ("Decades of research on scientific controversies have documented the limited nature of the 'organized' skepticism of science.").

88. See ROSE, *supra* note 29, at 185:

There is no prestige to be gained from simply repeating someone else's experiment; you are very unlikely to get a grant to do it, and the main scientific journals are not normally interested in publishing 'replications' of experiments unless they are on a particularly controversial topic. Even failures to replicate are not very interesting to the journals; experiments with negative results therefore rarely get reported.

See also Howard, *supra* note 56, at 45 ("[A] premium is placed on original research . . . [R]eplicating published experiments reaps little or no reward.").

89. See LATOUR, *supra* note 28, at 40 (noting that all scientific articles are not equal. "[M]ost papers are never read at all."). See also BEVERIDGE, *supra* note 41, at 3 ("[T]he utter impossibility of keeping abreast of more than the small fraction of the literature which is most pertinent to one's own research.").

90. HULL, *supra* note 50, at 435; cf. Black et al., *supra* note 34, at 762 ("The degree of corroboration does not depend simply on the number of tests, but rather on their variety and severity. Variety is required because performing the same test again and again provides little or no new information.").

91. See HULL, *supra* note 50, at 394 ("Scientists cannot spend very much time checking the work of other scientists if they themselves are to make contributions.").

usually needs an incentive to check another's work,⁹² and while such incentives may be only sporadically present, when such testing does occur it is meaningful. "They reserve checking for those findings that bear most closely on their own research, chiefly those that threaten it. Because different scientists are committed to different views, the checking that goes on in science rarely degenerates into empty show."⁹³

Confidence that knowledge is being advanced is highest not because of faith that research is unbiased, but when there is a diverse community containing scientists with different biases, for then incentives for checking results is at the highest.⁹⁴ Such a community, however, not only may check; it helps to insure that the researcher will attempt to do work in a way to transcend individual biases. The investigator who wants credit and influence in such a competitive community has strong motivations in doing the original research rigorously because he knows others who see the world differently may carefully scrutinize his results.

A scientist in a community, however, also seeks to do rigorous work because of allies. Such colleagues are unlikely to test the work for themselves, but, if it seems important, simply to incorporate it into their own research agenda.⁹⁵ Credit and influence wane if allies learn the work was fundamentally flawed. Allies learn this not by testing, but as a by-product of incorporation, for as the research gets assimilated into other systems, the work's flaws, if any, will under usual circumstances, eventually become apparent.⁹⁶ Finally, in a

92. *Id.* at 348 ("Although indiscriminate testing does take place in science, in general scientists must have some reason to test the findings that someone else publishes.").

93. HULL, *supra* note 50, at 394. *See also id.* at 348 ("Testing is reserved for findings that put one's own research into jeopardy.").

94. *See* HULL, *supra* note 50, at 22 ("One of the strengths of science is that it does not require that scientists be unbiased, only that different scientists have different biases.").

95. *Cf.* ROSE, *supra* note 29, at 186 ("What people do tend to do if a result someone else reports interests them is to repeat it with variants—that is, they test it in their own favourite animal or experimental situation.") *See also* HULL, *supra* note 50, at 348 ("Scientists incorporate into their own work those findings that support it, usually without testing.").

96. *See* HULL, *supra* note 50, at 342:

[S]tatements [scientists] take to be true . . . have to be integrated into theories, while those that are supposedly false are just dumped into a heap. Thus, within those systems of statements that scientists take to

community where scientists build on the efforts of others, scientists have to be adept evaluators. "Science is so structured that scientists must, to further their own research, use the work of other scientists. The better they are at evaluating the work of others when it is relevant to their own research, the more successful they will be."⁹⁷

Science, then, progresses and transcends mere social construction because of the interaction between a scientist and her scientific community. Without a diverse scientific community concerned with what a researcher produces, individual bias is not likely to be overcome.⁹⁸ If the researcher seeks influence and credit in such a community, however, she has strong incentives to do rigorous work. Because of the community, the work gets checked by those threatened by it and analyzed by those who seek to incorporate it. And if it does get widely incorporated in ways that ought to show the possible flaws, but they do not become apparent, we have good grounds for believing that the scientific claim is more than a construct.⁹⁹ Science as a community activity, then, is important because "the objectivity that matters so much in science is not primarily a characteristic of individual scientists but of scientific communities."¹⁰⁰

be true, errors should ramify. A theory of any scope at all includes hypotheses from a wide variety of sources. No one scientist can have firsthand knowledge of every sort of natural phenomenon that bears on his or her own research.

97. HULL, *supra* note 50, at 3-4.

98. *Id.* at 22 ("Because the only person available to test the beliefs of such isolated investigators is the investigator himself, individual bias is extremely difficult to eliminate.").

99. *Cf.* LATOUR & WOOLGAR, *supra* note 35, at 106 ("A fact becomes such when it loses all temporal qualifications and becomes incorporated into a large body of knowledge drawn upon by others."). See also Bert Black, *Review*, 1 SHEPARD'S EXPERT & SCI. EVIDENCE Q. 527, 528 (1994) (quoting HENRY H. BAUER, SCIENTIFIC LITERACY AND MYTH OF THE SCIENTIFIC METHOD 45-46 (1992)):

Bauer suggests that science is in many ways like a filter through which scientific knowledge is "gleaned from a mess of all sorts of suggestions, claims, and beliefs by progressive refining as errors and inadequacies are [culled] out." Frontier science—even published research—is not scientific knowledge until it has survived this filtering process. Only when other scientists start to use a new idea and build upon it—only when it becomes "text book" science—is the idea really "scientific knowledge." According to Bauer, textbooks are 90% right and journals are 90% wrong

100. HULL, *supra* note 50, at 3. See also Susan Haack, *Puzzling Out Science*, 8 ACADEMIC QUESTION 20, 26 (Spring 1995):

F. *Specialized Experience and Knowledge*

Science can work, as our common sense and daily experience reveal. Common sense, however, also tells that we can have learning without science. While scientists advance knowledge by, in essence, manufacturing encounters with nature,¹⁰¹ we also learn without such manipulation. We gain understanding from our normal experiences. When those experiences are shared by many, the resulting knowledge is ordinary common sense. Some people, however, also have experiences distinct from the common, and the learning derived from them is distinct or specialized knowledge to the rest of us. Those with such knowledge are also experts.¹⁰²

If, as we have seen, however, science can be social construction and affected by human limitations, biases, and misperceptions, then experiential learning must be subject to the

I doubt that real scientists are ever quite single-mindedly devoted to the truth; all, I expect, are motivated to some extent by the hope of fame or fortune, or to some degree in the grip of prejudice or partisanship. But to the extent that science is organized so as to maximize the likelihood that fame and fortune come to those who make real discoveries, or that partisans of one approach seek out the weaknesses which partisans of the other are motivated to neglect, a real community of imperfect inquirers can be a tolerable ersatz of an ideal community.

Cf. Heidi L. Feldman, *Science and Uncertainty in Mass Exposure Litigation*, 74 TEX. L. REV. 1, 10 (1995) ("While revised empiricists continue to regard a version of testability as one distinctive feature of science, they stress the definitive role of scientists' collective judgment in making testability work.").

101. *Cf.* Haack, *supra* note 100, at 25 ("All of us, in figuring out how things are, use, in Peirce's phrase, 'the method of experience and reasoning'; but science has, by [many] means . . . , enormously deepened and extended the range of experience and the sophistication of reasoning of which it avails itself."). See also BEVERIDGE, *supra* note 41, at 19:

[I]mportant as experimentation is in most branches of science, it is not appropriate to all types of research. It is not used, for instance, in descriptive biology, observational ecology or in most form of clinical research in medicine. However, investigations of this type make use of many of the same principles. The main difference is that hypotheses are tested by the collection of information from phenomena which occur naturally instead of those that are made to take place under experimental conditions.

102. See Imwinkelried, *supra* note 46, at 2289 ("Experience is to nonscientific experts as experimentation is to scientists."); Faust, *supra* note 40, at 35 ("Expert witnesses often make appeals to experience to back up their testimony. The persuasiveness of such appeals is understandable."); *cf.* Gross, *supra* note 6, at 1182 ("Expert evidence is based, to a greater or lesser extent, on systematic observations of replicable phenomena that are subject to independent study by trained observers.").

same possibilities.¹⁰³ If, as we have also seen, evidence law attempts to admit science when there are good grounds to believe it transcends construction, then evidence law should only admit claimed expert experiential knowledge when there are good grounds to believe that it, too, transcends individual and social limitations and biases.¹⁰⁴

Just as we need standards to judge when scientific expertise can be admitted, we need them also for expert experiential learning,¹⁰⁵ and evidence law demands that those standards be somehow equivalent to those used to screen scientific evidence. As Part III discusses, while *Daubert's* framework is of limited value in devising those general criteria, the fundamental premises of scientific advancement—rigorous encounters with nature, proof by disproof, and community activity that tests or incorporates—are principles that can, and should, be applied to expertise generally.

PART III

A. *The Daubert Factors*

Daubert's factors are not readily generalizable to non-scientific evidence. Just as important, however, they only partially capture the mechanisms that give confidence that science has advanced towards more knowledge. Thus, while falsifiability is a *Daubert* factor, and falsifiability an element of proof by disproof, more is needed than theoretical falsifiability for progress. Proof by disproof requires the rigorous testing in nature that allows confidence that individual

103. See Berndt Brehmer, *In One Word: Not From Experience*, 45 ACTA PSYCHOLOGICA 223, 225 (1980) (Research has shown "that people do not always learn from experience, at least not when the experience consists of a series of cases. This may be a startling conclusion and certainly one that goes against some of our most cherished beliefs: that experience improves our judgments and decisions.").

104. See Imwinkelried, *supra* note 46, at 2279:

[T]he trustworthiness of nonscientific expert testimony is every bit as suspect as the reliability of scientific evidence. If anything, there is less assurance of the accuracy and truthfulness of nonscientific expert testimony The lack of doublechecks calls into question the truthfulness as well as the accuracy of the nonscientific testimony.

See also O'Connor, *supra* note 69, at 275 (stating that scientists and experts believe they know positive truths "is irrelevant if these beliefs are not justified").

105. Cf. Imwinkelried, *supra* note 46, at 2283 ("[C]ourts have not developed objective standards to ensure the reliability of nonscientific expert testimony. Perhaps they have not done so because it cannot be done.").

limitations have been overcome. It requires not just a theory, but actions that make it likely that the errors in the investigator's conclusions, if any, would become apparent, and *Daubert* does not make this clear.

While touching on the essential proof by disproof, *Daubert* succeeds less in capturing the epistemologic role that the community plays in the scientific process. The Court referred to peer review, but this is of limited utility in overcoming social construction. While the often idiosyncratic, biased, and incomplete processes that constitute peer review may mean that others have examined a paper or proposal and concluded that the project and methods appear worthwhile,¹⁰⁶ they do not guarantee that the examination has been rigorously or skeptically done. Most important, peer review says almost nothing about the truly crucial community activities of testing or incorporation of the science.

Publication also has a limited value. Certainly without it, there can be little chance that others will test or incorporate the work.¹⁰⁷ Publication, however, hardly guarantees that that will happen. Publications are stoppered bottles cast adrift, seldom making it to shore. Much is published, too much for it to all be read.¹⁰⁸ And of that read fraction, the

106. See HULL, *supra* note 50, at 158-99 (discussing how haphazard and idiosyncratic peer review for publication may be; how little it may have to do with assessing quality and how much with the prejudices and viewpoints of editors and reviewers). See also JASANOFF, *supra* note 20, at 94-95 ("Peer review can take many forms, some far removed indeed from the ideal of neutral and dispassionate scientific inquiry [. . . and is the most important formal mechanism for enforcing scientific "boundaries."]). Cf. John Monahan & Laurens Walker, *Social Authority: Obtaining, Evaluating, and Establishing Social Science in Law*, 134 U. PA. L. REV. 477, 509 (1986) ("Research that has survived critical review is not necessarily trustworthy, but likely to of higher empirical quality than non-reviewed work.").

107. Cf. HULL, *supra* note 50, at 324 (Publication has "three functions: making discoveries public so that anyone who chose to could use them, awarding credit for the contribution to the author of the paper, and conferring some authenticity to the publication.") See also Black et al., *supra* note 34, at 776 (Science depends on the free exchange of accurately reported information.). Cf. LA-TOUR & WOOLGAR, *supra* note 35, at 47, 71 & 88:

[F]ar from being reports of what has been produced in the [scientific] factory, members take these papers to be the product of their unusual factory The production of papers is acknowledged by participants as the main objective of their activity Laboratory activity [is] the organisation of persuasion through literary inscription.

108. Cf. ROSE, *supra* note 29, at 302 ("[E]ventually even the weakest paper is likely to find a published home, though whether it will ever be read or referred to again is a different matter.").

scientific community only regards a fragment as important.¹⁰⁹ The crucial issue is not publication, but what happens after publication, and the usual response is nothing.¹¹⁰ The community only helps assure that science is more than a construct when others act upon the results, and publication is merely an initiating step in this crucial process.

Together peer review and publication, however, do indicate that the researcher is trying to be part of the scientific community and that she wants credit and influence with peers.¹¹¹ Such a scientist is aware that others may check results and hopes that others will use them. These outside forces give incentives to the scientist seeking validation within the scientific community that the isolated scientist does not have, prompting the former to do the research rigorously. Yet these incentives do not guarantee the necessary community action.

Finally, while general acceptance does seem like a quintessential community activity, it is one that actually gives little assurance that science has been lifted above being a construct. General acceptance is intrinsically a social activity, and what gets accepted in a scientific field is often highly socially determined. It depends upon "the negotiated agreements within a research community about a host of issues ranging from the applicable theoretical paradigm to norms of peer review and publication."¹¹² The true value of general ac-

109. See Rose, *supra* note 29, at 302 n.* ("A high proportion of papers, even in the most prestigious journals, are, it seems, never cited again [I]f you want to be read and noticed, it becomes necessary to say the same thing often and in many different places."); cf. LATOUR & WOOLGAR, *supra* note 35, at 252 ("[C]itation analysts have demonstrated the extensive waste of energy in scientific activity. Most published papers are never read, the few that are read are worth little, and the remaining 1 or 2 percent are transformed and misrepresented by those who use them.").

110. Cf. HULL, *supra* note 50, at 287 ("The chief weapon of the scientific to new ideas is a conspiracy of silence."). The lack of critical response after publication is no guarantor of reliability. Instead, the absence of a reaction probably indicates instead that the work was seen as so trivial or obviously suspect that it would have been a waste of effort to challenge it or incorporate it in another's research agenda.

111. Cf. HULL, *supra* note 50, at 155 ("Only those concepts that are transmitted can influence the development of science, and the primary vehicles for this transmission are scientists and their published works.").

112. Jasanoff, *supra* note 35, at 349. Cf. Haack, *supra* note 100, at 21-22 (emphasis added):

The basic strategy [of the New Cynics who claim science is a product of social interests without objectivity] is to shift attention from the nor-

ceptance varies tremendously because scientific fields employ diverse standards with greatly differing rigor as to what gets accepted in each.¹¹³ Most important, general acceptance does not guarantee that the community has acted in such a way to assure that scientific progress has been made.¹¹⁴ Once again, the crucial issue is not whether a scientific society has sanctioned something, but whether the community has tested it or incorporated it in such a way as to show any flaws.

B. A General Framework for Assessing Expertise

Although the *Daubert* factors are not useful for devising general standards for screening expertise, the scientific process still holds the epistemological key. Science makes progress by in effect confronting a crucial question. How likely is it that any errors in the scientist's belief would be known? There are good grounds for believing that the scientific assertion is more than a construct only when there is reasonable confidence that such flaws would become known and they have not. Each step in the scientific process can increase that confidence. Errors are likely to be known when reliance is placed not upon mere "logic" or assertions of authority, but instead nature is encountered. Flaws are even more likely to be found when reliance is not placed upon confirmatory evidence, but rigorous proof by disproof is sought. Faults are even more likely to be apparent when the isolated actor is disfavored, but a diverse community has tested and incorporated the scientific results. If a claim survives all these steps intact, there are good grounds for believing that knowledge has been advanced and the assertion is not just a construct.

This basic approach can, and should, be applied to expertise generally. Just as this process can give good assurances

mative notion of *warrant* (of how good the evidence is for this that scientific claim) onto the descriptive notion of *acceptance* (the standing of a claim in the eyes of the relevant community).

113. See LEWONTIN, *supra* note 44, at 491 (stating that the quality of evidence in different scientific fields is tailored to fit ideological demands); LATOUR & WOOLGAR, *supra* note 35, at 258 n.3 ("[T]he importance of somatostatin for the treatment of diabetes ensure that each of the group's articles is carefully checked. In the case of endorphine, by contrast, any article (no matter what the wildness of its conjectures) will initially be accepted as fact.").

114. Cf. David L. Faigman et al., *Check Your Crystal Ball at the Courthouse Door, Please: Exploring the Past, Understanding the Present, and Worrying About the Future of Scientific Evidence*, 15 CARDOZO L. REV. 1799, 1808 n.27 (1994) ("[S]ome portion of the 'knowledge' of established fields is invalid.").

that the scientific knowledge is not just a construct, it can give good assurances that other expertise is truly likely to be "knowledge."¹¹⁵ Before admitting any expertise, both the scientific and the nonscientific, courts should be grappling with the same issue: How likely is it that any errors in the beliefs upon which the expert testimony rests would have become known? Courts should be examining the encounters with nature that support the nonscientific expert opinion, probing whether the expert's experiences provide an adequate surrogate for rigorous scientific proof by disproof, and exploring whether community action truly helps assure that individual limitations, misperceptions, and biases have been transcended.

C. *Nonscientific Encounters with Nature*

Just as a scientist can only begin to transcend possible construction by testing conclusions against what happens in nature, the nonscientific expert, when describing the world, should have encounters with that world upon which to base an opinion. Often that is so obvious that we give it little thought. We would expect that the expert testifying about usual accounting practices to have encountered the accounting world. We would expect an expert testifying about a property's value to have encountered the world of property valuation. On the other hand, if the underlying premises of an economist are based on his or another's logic, assumptions, or theories instead of encounters with the world, courts should be quick to see that there are not good grounds for believing the "expertise" is anything more than a construct.¹¹⁶

115. See Haack, *supra* note 100, at 23:

Our standards of what constitutes good, honest, thorough inquiry and what constitutes good, strong, supportive evidence, are not internal to science. In judging where science has succeeded and where it has failed, in what areas and at what times it has done better and in what worse, we are appealing to the standards by which we judge the solidity of empirical beliefs, or the rigor and thoroughness of empirical inquiry, generally.

See also Weinberg, *supra* note 49, at 13 ("It seem to me that our science is a good model for intellectual activity.").

116. Cf. Levitt, *supra* note 57, at 285:

Many assumptions of normative law and economics are either untested or called into doubt by empirical testing. For example, economists assume that individuals make choices that maximize self-interest. Ex-

A recent case involving organized crime provides an example of where inquiries should have been made as to whether the expert had satisfactorily encountered the world he was testifying about.

At trial, Special [FBI] Agent Schiliro testified at great length on the nature of and function of organized crime families and disclosing the "rules" of La Cosa Nostra. For example, Schiliro testified that a "boss" must approve all illegal activity and especially all murders and that the functions of the "consigliere" and "underboss" are only "advisory" to the "boss." . . . Schiliro specifically named John Gotti as the boss of the alleged Gambino crime family and Gravano as the consigliere.¹¹⁷

Just as a scientist's beliefs must come from encounters with the nature described for confidence that the claims are more than constructs, the nonscientific expert's claims must also come from encounters with the world he testifies about. Here, apparently, the agent's relevant encounters with the organized crime world included many informers and surveillance tapes that had not been admitted into evidence.¹¹⁸ Whether the witness had the necessary or sufficient encounters was not truly considered by the court. Instead, the Second Circuit met the defendant's attack on these sources of information by stating that they satisfied Rule 703, which permits the expert to base an opinion upon non-admitted evidence "[i]f of a type reasonably relied upon by experts in the particular field."¹¹⁹

planations for this basic assumption, however, are sadly lacking Second, normative law and economics' derivation of legal solutions through cost-benefit analysis is far from an objective mathematical process Third, when legal facts or rules fail to comport with economic theory, efficiency theorists suggest that the rules should be changed to conform with the theory. This seriously limits the opportunity to disprove efficiency theory. Thus, law and economics theory fails to comport with the criterion of falsifiability.

Cf. also Daniel W. Shuman, *The Psychology of Deterrence in Tort Law*, 42 U. KAN. L. REV. 115, at n.132 (1993) ("[E]conomic analysis of tort law proceeds on many behavioral assumptions that are either not validated, or have been disproved by empirical research.").

117. *United States v. Locascio*, 6 F.3d 924, 936 (2d Cir. 1993).

118. *See id.* at 937-38 (stating that defendants contended that agent relied upon "countless nameless informers and countless tapes not in evidence . . ."). *See also id.* at 936 ("Schiliro admitted that his sources of information were not necessarily before the court.").

119. Schiliro was entitled to rely upon hearsay as to such matters as the structure and operating rules of organized crime families and the iden-

Even if the testimony comported with Rule 703, however, Rule 702 still had to be satisfied. The expert had to be presenting "knowledge," as Rule 702 requires, based upon "good grounds," as *Daubert* indicates. From the fragmentary report, it is not clear that the witness's encounters with the world he testified about were satisfactory to give him the requisite knowledge. Can a non-accountant testify as an expert about standard accounting practices based upon talking with a non-random sample of accountants and overhearing accountants even if such information were reasonably relied upon a field of experts generally? Was the organized crime expert different? Answers cannot be given because the basic Rule 702 questions were not addressed.

D. *Nonscientific Proof by Disproof*

Science has shown that simple confirmations of a theory are insufficient to support a conclusion that the claim is not a construct. We too easily see what we want to see. The same is true, of course, for all expertise, and thus it should not be sufficient for the expert merely to cite verifications of his premises.¹²⁰ Instead, the expert's experience has to be the kind that was likely to show that his premises were wrong if they were. Without that, the claimed knowledge is too likely to be a construct to be considered reliable.

The organized crime expert certainly raises this issue. At some point, because the agent has come to believe that the head of a crime family controls the actions of its members and that Gotti was the head of such an organization, the agent will naturally attend, in a biased manner, to verifications of his theory and avoid disconfirming information.¹²¹ Science

tification of specific voices heard on tape in forming his opinion, since there is little question that law enforcement agents routinely and reasonably rely upon such hearsay in the course of their duties.

Id. at 938.

120. Cf. Imwinkelried, *supra* note 46, at 2290-91 ("When a nonscientific expert witness cannot point to any experiences supporting the proffered opinion, the opinion is nothing more than conclusory conjecture.").

121. See Donald C. Langevoort, *Where Were the Lawyers? A Behavioral Inquiry Into Lawyers' Responsibility for Clients' Fraud*, 46 VAND. L. REV. 75, 100-02 (1993):

[I]f there is general agreement about any concept in social cognition, it is that of cognitive conservatism. [Searches of information and] of memory are biased, sometimes heavily, toward the confirmation of existing schema This research suggests strongly that under special

has shown, however, that we should not trust assertions unless the investigator was likely to have encountered possibilities of disproof. That epistemological point should be applied here, and thus we should ask of the organized crime expert, how would he have known if his conjectures were wrong? How would he have learned if members acted without authorization? How would he have perceived if someone else were the boss? His knowledge, if it was based solely on confirmatory incidents, is too likely to be the product of unwitting misperceptions and biases, is too likely to be a construct, to be trusted. Without the real possibility of encountering disproof, the conclusions of the nonscientific expert should be rejected just as a scientific expert's would be.

Scientists have the possibility of encountering disproof by doing controlled studies seeking what is theoretically forbidden, but controlled experiments are not always necessary: Experience can provide a comparable surrogate. The experience must be critically examined, however, for not all experience provides the opportunity for disproof. The experience must be the kind that gives consistent, repeated, and unambiguous feedback about possible errors.¹²²

For example, assume a claim that logs on a truck were incorrectly secured and thereby caused an accident. The defendant introduces evidence that the logs were secured in fashion X. A truck driver then testifies as an expert for the defense and states that in his opinion logs secured in fashion X are unlikely to come loose. Should that opinion be allowed? That truck driver-witness has not done controlled studies that support his opinion, but if his experience has included many instances of trucks driven with logs secured as described, he has good grounds for the opinion based upon his experience. There is a strong likelihood that if his opinion were based on a faulty premise he would know it. He would

circumstances, people will be motivated preconsciously to avoid the appreciation of adverse information, and hence fail to adopt appropriately vigilant cognitive modes. Their thinking turns wishful.

122. See *id.* at 99, n.99 ("[A] number of psychologists have concluded that for feedback to work, it must be unambiguous and, unless particularly salient, repeated."). Cf. Newman, *supra* note 63, at 184 ("[M]any clinical experts never obtain feedback on how their prior recommendations have fared. Without feedback, there is no opportunity to learn one's early mistakes . . .").

have suffered accidents.¹²³ Such experiences would furnish a very practical substitute for the controlled studies of science.

All experiences, however, do not give such unambiguous feedback about possible errors,¹²⁴ and without it, the claimed learning should be suspect. A recent case, *United States v. Starzecpyzel*,¹²⁵ about handwriting experts provides an example of a court failing to make distinctions about the learning potential of different kinds of experiences. The court first determined that appropriate validation research for this expertise had not been done.¹²⁶ Forensic document examination, consequently, is not scientific knowledge within the meaning of *Daubert*.¹²⁷ The court, however, accepting the dual-level approach to expertise,¹²⁸ concluded that the handwriting expert, as a nonscientific, technical expert, could still testify because the witness was qualified and had technical knowledge that could assist the jury.¹²⁹

123. *Cf. Hughes v. Hemingway Transp.*, 539 F. Supp. 130 (E.D. Pa. 1982). Defendant's tractor-trailer, which skidded during icy conditions, injured the motorcyclist-plaintiff. *Id.* The court held that defendant's proffered expert, who claimed expertise on the proper techniques for controlling a skidding truck during icy conditions acquired "exclusively through training provided by his employer United Parcel Service," was not qualified to testify. *Id.* at 133. "Specifically, Mr. Walsh testified that he did not have either a scientific or engineering background; that he was unable to calculate the coefficient of friction on the roadway at the time of accident." *Id.*

The true issue, however, was the proffered expert's technical education. "One need not be an engineer . . . or know how to calculate the coefficient of friction to know how to drive a truck on ice." Murphy, *supra* note 6, at 641. The issue should have been whether Walsh's experiences had given him clear and repeated feedback on proper and improper truck-driving under icy conditions.

124. *See, e.g., Faust, supra* note 40, at 37 ("[I]n many instances experience can be downright misleading. The feedback provided by experience in psychiatry is exceedingly complex and ambiguous.").

125. *U.S. v. Starzecpyzel*, 880 F. Supp. 1027 (S.D.N.Y. 1995).

126. *Id.* at 1037 ("[T]he Court is not aware of any substantial argument that proper validation testing cannot be conducted.").

127. *Id.* at 1038 ("[F]orensic document examination, despite the existence of a certification program, professional journals and other trappings of science, cannot, after *Daubert*, be regarded as 'scientific . . . knowledge.'").

128. *Id.* at 1040 ("The Court therefore finds no support for the proposition that *Daubert* extends past the 'scientific' branch of Rule 702 to other forms of expert testimony.").

129. *Id.* at 1043 ("[T]he witness must possess such relevant form of '[technical or other specialized] knowledge.' Second, the knowledge must 'assist the trier of fact.' Finally, the witness must be 'qualified as an expert.'").

The court's common sense accepted that various handwritings have significant similarities and differences,¹³⁰ and that from these characteristics the source of the handwriting could be determined.¹³¹ Through experience, the court asserted, forensic document examiners can become proficient in these determinations.

In a nutshell, over a period of years, FDE's gradually acquire the skill of identifying similarities and differences between groups of handwriting exemplars. Such expertise is similar to that developed by a harbor pilot who has repeatedly navigated a particular waterway. The Court therefore treats forensic document expertise under the "technical, or other specialized knowledge" branch of Rule 702, which is apparently not governed by *Daubert*.¹³²

As a result, reliable expertise, although not demonstrated, is possible, and handwriting expertise ought to be admitted. "[E]xperienced individuals may be able to detect 'significant' similarities and differences between sets of handwriting exemplars [R]eliable determinations of genuineness are possible The Court therefore finds sufficient indicia of reliability to sustain the admissibility of FDE expertise as nonscientific expert testimony."¹³³

At first glance, this seems to make sense. Handwritings are not all the same, and the variations in them seem a way

130. *Id.* at 1044 ("Although Ms. Kelly [the prosecution's witness supporting the reliability of forensic document examination] was unable to explain to the Court's satisfaction precisely how 'significant' similarities or differences were identified, the Court has no doubt that such identifications can be performed, in some case by cursory examination.").

131. *U.S. v. Starzecpyzl*, 880 F. Supp. 1027, 1046 (S.D.N.Y. 1995) ("Even in as limited a class of writings as signatures, there are a large number of possible points of comparison As a matter of simple logic, the greater the number of points of comparison, the greater the certainty of the determination as to genuineness.").

132. *Id.* at 1029.

133. *Id.* at 1046-47. The crucial question is not whether experts can identify handwriting, but can they do it better than jurors. *Id.* The court's common sense said the experts, because of motivation and experience, would perform better than jurors. *Id.*

First, while jurors may have the ability to locate significant similarities and differences between sets of writings, it is not clear that they will, or should, take the time to conduct such comparisons *de novo* during a trial Second the Court does not doubt that, as with most tasks, skill increases with experience, so FDE's are likely to do a better job than even highly motivated jurors.

Id. at 1045.

to distinguish a cursive's source. Someone who regularly examines writings ought to be better than the rest of us at making such distinctions even though scientific studies have not been done. Surely we do not doubt that the harbor pilot has specialized knowledge that we lack, and the forensic document examiner seems similar.

That similarity, however, may only be superficial. Both the harbor pilot and the FDE may have extensive experience, but their experiences may not be equal teachers. The court should analyze how likely it was that the experiences would have revealed any mistakes or errors in the expert's methods.

The harbor pilot's experiences have given him expertise. At least as indicated about river boat pilots in Twain's *A Life on the Mississippi*,¹³⁴ every time the pilot ventures through the harbor he gets instantaneous feedback about his decisions. He navigates successfully or he runs aground. We can have high confidence that he would have learned about mistakes if he committed them. As a result, when he tells us this is a safe way to traverse this waterway, we can have confidence in the assertion.¹³⁵

The handwriting expert's experiences do not seem to have this same crucial component. When the handwriting analyst determines that Jones did or did not pen the letter, she does not get unambiguous feedback about her decisions. Handwriting determination is not "an area, like practical plumbing, in which the results of correct or incorrect practice would be obvious to bystander and practitioner alike. Rather, it is exactly the kind of situation in which it is easiest to fool oneself and others."¹³⁶ In the normal course of her business, at best she might confront another "expert" that reached another conclusion,¹³⁷ but she, quite naturally, could still maintain faith in her abilities and dismiss the other opinion. She

134. See MARK TWAIN, *A LIFE ON THE MISSISSIPPI* Ch. 6-12 (1899).

135. Cf. Weinberg, *supra* note 49, at 12:

[I]f a party of mountain climbers . . . argue about the path to some peak [t]heir arguments are conditioned by the social structure of the expedition, but when they find the right path they know it because then they get to the peak. No one would write a book about mountaineering with the title *Constructing Everest*.

136. D. Michael Risinger et al., *Exorcism of Ignorance as a Proxy for Rational Knowledge: The Lessons of Handwriting Identification "Expertise"*, 137 U. PA. L. REV. 731, 739-40 (1989).

137. See Irby Todd, *Do Experts Frequently Disagree?*, 18 J. FORENSIC SCI. 455 (1973) (explaining that such experts, however, have been rarely opposed).

would ordinarily never seem to confront the empirical world in a way that unambiguously could prove her wrong.¹³⁸ If that is so, there are not good grounds for believing her knowledge is more than a construct.

If courts are truly to admit only expert knowledge based on good grounds, courts must carefully analyze claims of experiential learning. Since humans are so easily prey to self-deception, even plausible explanations of how experience would have demonstrated the witness's premises wrong, if they were, should be skeptically probed. Science has learned that a common defect of human reasoning is to too readily assume a causal relationship from a temporal sequence.¹³⁹ If A occurs, and then B happens, we too easily conclude A causes B. The dangers are especially acute when we perform A and we want B to occur. If we believe that we can cause B, we naturally attend to the confirming proof of the temporal sequence and become convinced of our conjecture.¹⁴⁰ That, however, is not sufficient for giving good grounds for the claimed knowledge. The crucial question needs asking: How would the experience have shown the premise wrong if it were? Without a satisfactory answer, the claim should not be accepted as "knowledge."

Many areas might illustrate such possible flawed knowledge.¹⁴¹ Medicine is one of them. The doctor believes that a

138. J. Harris, *How Much do People Write Alike?: A Study of Signatures*, 48 J. CRIM. L. & CRIMINOLOGY 647 (1958) (stating that a handwriting expert who actually looked for signatures from different people that were indistinguishably similar found many examples).

139. See BEVERIDGE, *supra* note 41, at 154:

The most notorious source of fallacy is probably *post hoc, ergo propter hoc*, that is, to attribute a causal relationship between what has been done and what follows . . . All our actions and reason are based on the legitimate assumption that all events have their cause in what has gone before, but error often arises when we attribute a causal role to a particular preceding event or interference on our part which in reality had no influence on the outcome observed.

140. See Faigman, *supra* note 7, at 1055 ("A researcher's bias, although perhaps benign, may manifest itself through selective attention to the expected behavior, thus operating as a self-fulfilling confirmation of the hypothesis.").

141. See, e.g., Ambrose Bierce, THE DEVIL'S DICTIONARY, in THE COLLECTED WRITINGS OF AMBROSE BIERCE 229 (1970):

EFFECT, *n.* The second of two phenomena which always occur together in the same order. The first, called a Cause, is said to generate the other—which is no more sensible than it would be for one who had never seen a dog except in pursuit of a rabbit to declare the rabbit the cause of the dog.

certain treatment works on a certain condition. He gives it, and the patient gets better. He concludes that his treatments effects the cure. If we are to have good grounds, however, for this claim, we need to know how this experience would reveal that the assertion was wrong if it were. The answer seems obvious: If the treatment did not work, the patient would not get better. Certainly, if the patient did not improve after the treatment, we would have good grounds for believing that the treatment was inefficacious.¹⁴² The converse, however, is not true. If the patient gets better, how does the doctor know that something other than the treatment did not effect the improvement? Indeed, as the do-no-harm principle indicates, patients often improve without treatment. As long as the doctor's experience consists only of the confirmatory proof of treatment followed by cure, we can have little confidence that the assertion that "treatment causes cure" is anything more than a construct.¹⁴³

D. *Nonscientific Community Action*

Science teaches that we can be most confident that science has advanced knowledge when a community has tested or incorporated a claim in ways to show potential flaws. This is most likely to happen when there is a group with diverse biases who can be threatened by the work of others or seek to build upon it. The isolated scientist should be regarded skeptically, and we should have just as much doubt about the isolated expert.¹⁴⁴ Just because a group of people claim the

142. Cf. SAKS & VAN DUIZEND, *supra* note 30, at 74 ("A scientific or professional field has a powerful defense against unsubstantiated or exaggerated conclusions: replication. Professional/technical field that apply such knowledge have a 'defense'; erroneous principles will be found out because patients will not get well, planes will not fly, or chemicals will not synthesized.").

143. Cf. Brehmer, *supra* note 103, at 236:

If a psychologist invents a new therapy, he generally gives it to those who need it the most. Almost inevitably, he will now find they have improved and he will be led to conclude that his treatment was useful The problem is that there is a very likely alternative explanation: that the improvement he observed is simply a regression effect.

See also August Piper, Jr., *Multiple Personality Disorder and Criminal Responsibility: Critique of a Paper by Elyn Saks*, 22 J. PSYCHIATRY & L. 7, 35 (1994) ("[T]hat a patient improves under a physician's treatment says nothing about the accuracy of the diagnosis.").

144. See LATOUR, *supra* note 28, at 152-53:

[A]n isolated specialist is a contradiction in terms. Either you are isolated and very quickly stop being a specialist, or you remain a special-

same expertise, however, it does not mean that an expert is part of a community that can help assure that claims of knowledge transcend construction. The community's actions need to be analyzed to see if that community truly tests or meaningfully incorporates the work.

Harbor pilots, for example, might be part of a community whose actions can help lift learning above construction. At least Mr. Bixby, Twain's Mississippi mentor, was part of a community of river boat pilots who shared their knowledge with each other.¹⁴⁵ If one stated that a particular point was a safe place to cross a channel, others could be expected to follow. Pilots, concerned about their standing in this community and planning to build on the knowledge of others, had reasons to be circumspect in claims. More important, the community incorporated the knowledge and thereby tested it in a practical way by finding out if passage could be safely made as claimed.¹⁴⁶

Handwriting expertise again seems different. *Starzecpyzel* concluded that handwriting identifications are based not on publicly articulable standards, but apparently individualized criteria.¹⁴⁷ Even if such identifications did lead to feedback about possible errors, a community would not be able to test or incorporate the methods of others because that method could not be meaningfully communicated to it.¹⁴⁸ The kind of community action that can help tran-

ist but this means you are not isolated. Others who are *as specialised as you*, are trying out your material so fiercely that they may push the proof race to point where all your resources are barely enough to win the encounter. . . . But as long a research in internal combustion, neuroendocrinology, geology or chip design does not yet exist as a job, there is no specialist inside and no interested groups outside.

Cf. Robert J. Levy, *Using "Scientific" Testimony to Prove Child Sexual Abuse*, 23 FAM. L.Q. 383, 395-96 (1989) ("The child sexual abuse expert group tends to be socially and professionally isolated, unlikely to learn from professional colleagues in other fields.").

145. See TWAIN, *supra* note 134, at Ch. 15.

146. Similarly, the hypothetical truck driver's expertise is more reliable if not only he, but a community of truck drivers, secured their loads in the described fashion with reports circulating among them about problems caused by the loads.

147. U.S. v. *Starzecpyzl*, 880 F. Supp. 1027, 1046-47 (S.D.N.Y. 1995).

148. Cf. Nijboer, *supra* note 49, at 178 ("[I]t is possible to speak of expertise only if there is a degree of consensus about the contents of the specialty and its methods. Ideally, the professionalism of the expert is institutionalized, with the result that the object and methods of the science in question are clearly specified.").

scend construction does not seem possible in such a field.¹⁴⁹ Furthermore, in a field like this, we can expect that individual's conclusion to be derived in a less rigorous manner than in a field where there is meaningful community action. Where others cannot test or incorporate the work, a prime motivation for careful observation and inference has been removed from the individual expert.

Just as we can learn from experience, but not all experiences are good teachers, communities can help assure that knowledge has good grounds, but not all community action produces that assurance.

E. *The Effects of Assessing Expertise*

We can only speculate about the effects of truly assessing nonscientific expertise for confidence that it is based on good grounds. Courts have not routinely been making such assessments. While courts may usually be considering whether an expert has encountered the world he testifies about, courts less often seem to have been examining the expert's experience to see if it furnishes an acceptable surrogate for scientific proof by disproof. And, I believe, even more infrequently, if at all, have courts examined whether there has been community action that effectively tests or incorporates a nonscientific expert's claims in ways that would show flaws.

Until these analyses are regularly made, we cannot know the possible range of experiences and communities of expertise that should lead to confidence that the claimed knowledge is truly based on good grounds. Such assessments, however, will probably result in the exclusion of some expertise now admitted. This can be beneficial. Without some experts, trials might be less expensive and more efficient. And perhaps when suspect experiential learning is excluded, there will be incentives not now existing to do the rigorous studies that could give good grounds for the claimed knowledge. The result then would be better information to determine the facts upon which we base our justice.

149. Cf. Levine, *supra* note 29, at 64 ("The canons of method require that we admit as scientific only that which is public and communicable [E]xperiments in psychology are not entirely communicable and . . . the findings are therefore not truly public and may not be admissible as science.").

CONCLUSION

Since scientific evidence must be assessed to see if it is based on good grounds, all expertise ought to be similarly assessed. Scientific claims can transcend construction when they come from encounters with nature; when confirmatory proof is not relied upon, but rigorous proof by disproof is sought; and when a community has tested or incorporated the claim in ways to show possible shortcomings. All expertise should be subjected to a similar analysis.

